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MATHEMATICAL PHYSICS.

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Howard Higbee.

MATHEMATICAL PHYSICS;

OR THE

MATHEMATICAL PRINCIPLES

OF

NATURAL PHILOSOPHY:

WITH

A DEVELOPMENT OF THE CAUSES

OF

HEAT, GASEOUS ELASTICITY, GRAVITATION,

AND OTHER GREAT PHENOMENA OF NATURE.

nsisting of a series of propositions
monstrated mathematically.

BY JOHN HERAPATH, ESQ.

VOL. II.

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ERRATA.

Page 199, line 12, *for* 53°·6, *read* 53°·06.
302, *Cor.* 5, first line, *for* θx^n *read* $\theta d^{-n} x^n$.

DIRECTIONS TO BINDER.

Plates 1, 2, 3, at end of Vol. I.
The other Plate and the Tables at the end of Vol. II.

MATHEMATICAL PHYSICS.

BOOK III.

SECT. I.

MECHANICAL DIFFUSION OF GASES.

General Observations.

When airs are confined in glass or other vessels, having small cracks in them, some very curious phenomena occur, which, a few years back, were successfully studied by Professor Graham. It matters not what the character of the confined air is, light or heavy, it gradually flows out, and the air on the outside as gradually flows in. If the air outside is very great in bulk compared with the quantity imprisoned, verging towards infinitely greater, for instance, the whole air within will escape, and the vessel be filled exclusively with the external air.

So far, one might have guessed the phenomena from the experiments of Dalton in the communication of gases, through a small tube or orifice. He found that any two airs inclosed in vessels so connected, would in a short time equally intermix, without any regard to the laws of gravity. If the vessels were placed one over the other, the heavier gas would ascend and the lighter descend, or *vice versâ*, with equal indifference. Gravity seemed to have no influence at all. Professor Graham has, however, carried the subject much farther. He has shown, in a series of very beautiful experiments, that each gas flows in or out according to laws depending entirely on its specific gravity, without respect to any other quality of the gas. So accurately does the gas obey the law, that its specific gravity may be determined by the circumstances of its percolation or effusion through the crack, as correctly, he says, as by weighing it by the nicest balance.

If two or more gases are mixed together, every one follows the law peculiar to itself, regardless of the presence of the others. In this case, however, the aperture, if it be an aperture, must be so fine that the airs can keep themselves constantly adjusted. The larger the aperture the less likely the law is to hold good.

The instrument employed by Professor Graham was a graduated tube, closed or open at one end, and closed at the other by a plug of plaster of Paris, or some other porous substance. This instrument he calls a "*diffusion tube.*" When a tube of this sort is filled with any gas lighter than air, hydrogen, for instance, and the open end is immersed in water, the upper plaster of Paris end being dry, the hydrogen will rapidly escape, and the water, following the vacuum occasioned by the departing hydrogen, will rise up into the tube to the extent of many inches. "In a tube 14 inches long it will ascend 6 or 8 inches in as many minutes." After some time the air, which flows in less fast than the hydrogen flows out, will begin to fill the tube, and the water consequently to descend until it reaches its former level.

It will be the business of this section to demonstrate the laws and phenomena of airs under such circumstances, and particularly those observed by Professor Graham, who is the only labourer that I know of in this interesting branch of experimental physics. In doing so, it will be shown that heat has an important influence in the rate of diffusion, a fact that seems to have escaped the observation of the Professor, and very possibly to have disturbed some of his experiments. New experiments, therefore, embracing this element are desirable.

In the whole of the following observations it is understood, when speaking of mixed airs, that they are mere mechanical mixtures, and that no chemical action takes place between the airs, or the airs and any of the bodies. It is also understood that every air is perfectly homogeneous, that is, that all its particles are of the same mass, and that there is no action, chemical or other, between the membrane or pores through which the effusion takes place and the gas.

It is necessary thus to limit the phenomena, because there are cases of diffusion of a very different description. Thin films of water, caoutchouc, and humid membranes exercise a remarkable influence on the passage of certain gases. The more easily gases are liquefiable, by compression or cold, or both, the more readily, in general, they permeate these substances; and the more difficult they are of liquefaction, the less easily they pass. According to Dr. Mitchell, the following are the times occupied by equal volumes of different gases passing through the same membrane:

Ammonia	1	minute.
Sulphuretted hydrogen	2½	„
Cyanogen	3¼	„
Carbonic acid . . .	5½	„
Nitrous oxide . . .	6½	„
Arsenietted hydrogen .	27½	„
Olefiant gas	28	„
Hydrogen	37½	„
Oxygen	113	„
Carbonic oxide . . .	160	„

" And a much greater time for nitrogen." (Graham's 'Elements of Chemistry.')

This irregular, or rather partial, diffusion appears to have been first noticed by Dalton, who observed that carbonic acid gas passed through a film of water, and intermixed with and expanded through a confined portion of air on the other side of the aqueous film.

There is no doubt that this phenomenon has been rightly attributed by Mr. Graham to the liquefiable disposition of the several gases with the membranes and bodies through which they have to pass. It is a phenomenon analogous to the absorption of gases by water; but there is great doubt of the airs being actually liquefied by the membrane, and re-evaporated on the other side, as Mr. Graham imagines. It appears more probable that particles of the gases pass slowly through the film or membrane in an unliquefied state, by a means which may be hereafter explained. However, further observations are wanting to determine the precise manner of the passage. The phenomena are certainly not the same, nor possibly akin to the diffusion proper of gases. It appears, however, to be a subject which, thoroughly explained, will likely enough be found to comprehend matters of considerable interest.

PROPOSITION I.

The number of particles which strike against a given portion of the containing surface of any air, is, in a given time, as the elasticity directly and temperature inversely, that is, as $\frac{E}{T}$.

Though we have deduced this Proposition in *Cor.* 5, Proposition VIII, Section 2, Book II, p. 261, its importance is so great in the subsequent inquiries that I offer no apology for an independent proof, particularly as we shall subjoin in its corollaries some more general details.

For the elastic force is as the number of collisions in a given time and the force of each conjointly, that is, as the number and temperature, and therefore if C denote the number of the said collisions,

$$E \propto TC \text{ and } C \propto \frac{E}{T} \quad . \quad . \quad . \qquad (1).$$

Cor. 1. This is true whatever be the character of the airs, light or heavy, homogeneous or not, simple or mixed. So that if the elasticity and temperature be the same, the number of collisions is the same in all airs.

Cor. 2. Hence if the temperatures are equal, the numbers of particles of any two airs whatever, striking against equal spaces in the same or equal times, are directly as the elasticities.

Cor. 3. If the volume remain the same the elasticity is as T^2, and therefore the number that strike against a given space in a given time, is as T.

PROPOSITION II.

The times of percolation into vacua of given volumes of gases through small apertures, kept each at a uniform temperature and elasticity during the time of percolation, are as the volumes and square roots of the specific gravities directly, and the simple of the temperatures inversely; that is, as $\frac{V\sqrt{G}}{T}$.

For the numbers of particles run out in a given time, are as the numbers that strike against a given space in that time; that is, by the preceding Proposition, as $\frac{E}{T}$. And the time a given volume is running out, is as the number in the volume directly, and the number that flows out in a given time inversely. But the number in the volume is as the volume V and the number in a given space, that is, as the volume V, the square root of the specific gravity G, the elasticity E all directly, and the square of the temperature T inversely. Therefore the time of percolation or

$$\tau \propto \frac{VE\sqrt{G}}{T^2} \times \frac{T}{E} \propto \frac{V\sqrt{G}}{T} . \quad . \quad . \quad (2).$$

Cor. 1. If therefore the volumes and temperatures of two different airs be the same, the times of percolation are simply as the square roots of the specific gravities, without any regard to the elasticities.

Cor. 2. Hence in the same gas the proportion or part of a given volume run out in a given time, is the same whatever be the elasticity, provided the temperature is the same. This is easily proved otherwise. For the number of particles striking against a given area in a given time, is as the number in the given volume.

Cor. 3. If therefore there be different quantities of the same gas confined in equal volumes, and there be equal apertures, the two volumes will empty themselves in the same time whatever be the inequality of elasticity, provided of course that the temperatures are equal. For the momentary leakages bear the same proportion to the quantities remaining, and therefore in equal times the same proportional quantities will be abstracted; and the same proportional quantities to the first whole quantities remain.

Cor. 4. This draws a strong line of demarcation between the percolation of the same and different gases. Let us, for example, suppose two gases confined in equal volumes of the same temperature, and that the lighter is so compressed as to be of the same weight as the heavier, the times of percolating out will nevertheless not be the same. But the times will always bear the same ratio to each other, whatever be the quantities of the gases contained in the same volumes, as long as the temperatures are the same, namely, the square roots of the specific gravities of the two airs.

Cor. 5. We have here been speaking of perfectly homogeneous gases. If the gases are not homogeneous the law will not strictly hold. We shall therefore be enabled to discover by the times of percolation whether the airs are homogeneous or not. If the times agree with the theory they are homogeneous, but not otherwise. We have hence a method of examining the purity of a gas; for I presume a gas to be pure must be homogeneous, and if it is not, it is a mechanical mixture.

Cor. 6. Hence if we can make the experiment accurately, we may decide a great and much disputed question, namely, whether the atmosphere is a mechanical or a chemical mixture of oxygen and nitrogen. For suppose we get a gas perfectly pure, and compare the times of its percolating out one half, or any other part of the whole, with the times of the air percolating, if the times in the air and gas follow the law of the square roots of the specific gravities, it is manifest the air is a chemical compound, but if not it is a mechanical mixture. This will be recurred to hereafter in a more certain and easily managed experiment.

Scholium.

When the preceding proposition and its corollaries were drawn up, March 17th and 18th, 1844, as per date of MS., no experiments that I am aware of existed to prove or disprove them. I however felt so entire a confidence in the truth of the principles, that I hesitated not to let them stand as a part of this section. Indeed, the first two sections of the first volume having been, on December 12th, 1846, delivered to the printer, I commenced on this day re-reading this second volume, for the purpose chiefly of punctuation, and had passed over and punctuated this entire section before I read and considered what Professor Graham had published, November 1846, in the first part of the second edition of his Chemistry. Until in fact I read anew this section, I had a very imperfect recollection of what I had done upon the subject above two and a half years before, and did not remember what had been proved by experiment, and what remained to be proved. The moment, however, I read (December 13th) the new experiments Mr. Graham appears to have made, to which I shall presently allude, I felt it right again to call my son Mr. Edwin Herapath to witness the complete anticipation I had given of them so long ago. Knowing the unkind observations that are sometimes made, I feel it necessary to take these precautions, and to publish them, to save parties the trouble of making unfounded remarks.

It was my intention to communicate to Mr. Graham the results at which I had arrived, as a guide to him in future experiments, but as he did not think proper to answer some questions I sent him, I did not deem it worth while. But to the experiments.

Mr. Graham, p. 80, put "a jar on the plate of an air-pump," which "is kept vacuous by continued exhaustion, and a measured quantity of air, or any other gas, allowed to find its way into the vacuous jar through a minute aperture in a thin metallic plate, such as platinum-foil, made by a fine steel point, and not more than 1-300th of an inch in diameter." * * * * "By a perforated plate such as described, 60 cubic inches of dry air entered the vacuous, or nearly vacuous, air-pump receiver in about 1000 seconds, and in successive experiments the time of passage did not vary more than one or two seconds."

"The time of passage into a vacuum of a constant volume varied in the different gases, the lightest passing in the shortest time. The time corresponded very closely for each gas with the square root of its density."

He then mentions four experiments made at different times, the extremes of which are 1·0519 and 1·0502, and the mean 1·05115, for the passage of oxygen, that of air being 1. The square root of the specific gravity of oxygen, that of air being 1, is 1·0515, which should show the time of effusion. The times of other gaseous passages are given as follows with regard to oxygen being 1, not air 1.

Hydrogen	·2631	instead of	·2502
Nitrogen	·9355	. . . .	·9373
Carbonic oxide . . .	·9345	. . . .	·9354
Carburetted hydrogen	·7023	. . . .	·7071
Carbonic acid . . .	1·1675	. . . .	1·1760
Nitrous oxide . . .	1·1675	. . . .	1·1705

The square roots of their specific gravities to oxygen 1.

"The sensible deviations," he observes, "from the law in the times of both the very light and very heavy gases, can be shown to be occasioned by the tubularity of the aperture, arising from the unavoidable thickness of the metallic plate."

A part of the deviations from the law may probably be due to the gases percolating not into a perfect but an imperfect vacuum. The closeness, however, of the observed numbers, with those that should hold according to the square roots of the specific gravities, is a proof of the law in the preceding proposition and its corollaries.

At p. 84, Mr. Graham observes upon some experiments, that "the effusion of a given measure of air into a vacuum takes place always in the same time, whatever may be its density from one fourth of an atmosphere up to two atmospheres."

This is precisely the law laid down in our third corollary, nearly two years and three quarters before.

He continues, "but the transpiration" (passage through a tube) "of air of different densities, was observed to take place in times which are inversely as the densities; or, the denser the air is, the more rapidly is a given volume of it transpired."

Our *Cor.* 4 comprehends the fact here stated in much more general terms. It is there shown, speaking of the same air, of course at the same temperature, that it is the volume, not the density or elasticity of the gas, which determines the time of its effusion or percolation. And, from the principles of the law, the air, however dense and elastic, does not rush out any faster, or with any greater velocity of the particles, but only in greater numbers. Mr. Graham imagines that "the higher the barometer the more quickly the gases transpire;" and he continues, "the difference in this respect separates completely the phenomena of effusion and transpiration."

This is erroneous. There is no difference whatever between his "*diffusion*," "*effusion*," and "*transpiration*," except in so far as friction through the tubes in the latter case may influence the result. We have deduced the whole from the same simple principles, and comprehended them in the same law, which could not be done if, as he imagines, *effusion* and *transpiration* were different properties, or the latter "a constitutional property, like density, or combining volume." He may rest assured, if he continue the investigation of the supposed "constitutional property," with any hope of a "peculiar interest from supplying a new class of constants for the gases," he will only waste his time. There is nothing new in it, beyond what he and others knew many years ago, and beyond what I could and would have told him within a few hours after I took the subject in hand in March 1844.

I trust it will not be supposed that these observations are intended to throw any discredit over the learned Professor's labours, which are very creditable to his skill and industry, and for which I, as one of the public, feel deeply indebted to him. It is merely wished to dissipate the error of a supposed discovery of any new property of airs or gases in this "transpiration."

We may presently have to allude to another point in which Mr. Graham has been unfortunate in his conclusions, and has imagined he has discovered a new property in the case of hydrogen.

The following Propositions I have added in consequence of the turn the Professor's experiments have taken. I have chosen to distinguish them by letters, and to insert them here, rather than to disturb the order of the Propositions that follow.

PROPOSITION A.

Let G, g, *where* $G = rg$, *be the specific gravities of any two gases mechanically mixed in the proportions, as to volume, of* V *to* v*; and let* V, v *denote the proportional volumes of the two airs remaining in the reservoir* *after the mixture has percolated through a very small aperture under a constant pressure during any given time; then will*

$$\left(\frac{\mathbf{V}}{V}\right)^{\sqrt{r}} = \frac{\mathrm{v}}{v} \quad . \quad . \quad . \quad (a)$$

assuming the airs to percolate without disturbance from each other.

Suppose P, p to denote the numbers of particles of the airs respectively remaining at the given time in the reservoir, and E, e their elasticities at that time. Then dP, dp will be the numbers of particles of each that flow out simultaneously. But by Proposition I,

$$dP : dp :: E : e :: V : v;$$

and, by the laws of gases,

$$P : p :: V\sqrt{G} : v\sqrt{g} :: V\sqrt{r} : v.$$

Consequently,

$$\left(\frac{\mathbf{P}}{P}\right)^{\sqrt{r}} = \frac{\mathrm{p}}{p},$$

if we take $\mathbf{P}$, p to denote the original number of particles in the reservoir.

But since $\mathbf{P} : P :: \mathbf{V} : V$

and $\mathrm{p} : p :: \mathrm{v} : v$,

the equation just found may be transformed into

$$\left(\frac{\mathbf{V}}{V}\right)^{\sqrt{r}} = \frac{\mathrm{v}}{v}, \text{ or } \left(\frac{V}{\mathbf{V}}\right)^{\sqrt{r}} = \frac{v}{\mathrm{v}}.$$

Cor. 1. Since, if G is greater than g, r always exceeds unity, $\frac{\mathbf{V}}{V}$ is therefore always less than $\frac{\mathrm{v}}{v}$; or a greater proportional quantity of the lighter air flows out in the same time, and a less proportion remains in.

Cor. 2. By the very same arguments we find if

$$V, V_1, V_2, V_3,$$

represent the volumes of different gases at any common time in the mixture; and

$$\mathbf{V}, \mathbf{V}_1, \mathbf{V}_2, \mathbf{V}_3,$$

the volumes of the corresponding gases at the beginning of the percolation; and if

$$G, G_1, G_2, G_3,$$

be their specific gravities; then will

$$\left(\frac{\mathbf{V}}{V}\right)^{\sqrt{G}} = \left(\frac{\mathbf{V}_1}{V_1}\right)^{\sqrt{G_1}} = \left(\frac{\mathbf{V}_2}{V_2}\right)^{\sqrt{G_2}} = \left(\frac{\mathbf{V}_3}{V_3}\right)^{\sqrt{G_3}} = \dots \quad (b).$$

PROPOSITION B.

Of two gases mechanically mixed in given proportions as in the foregoing Proposition, it is required to find the time of percolation of a given quantity from the reservoir, the elasticity **E** *upon the mixture being constant.*

Using the same notation, and putting τ for the time elapsed, we have

$$a \frac{E}{T} d\tau = -dP \text{ and } a \frac{e}{T} d\tau = -dp.$$

Whence, adding the two together and integrating, we have

$$\frac{a\,\mathrm{E}\,\tau}{T} = \mathrm{P} - P + \mathrm{p} - p.$$

In these equations a is a constant quantity, to be determined hereafter, depending chiefly on the size of the aperture.

Now because $\mathrm{P} = \frac{\mathrm{V}}{V}\,P$ and $\mathrm{p} = \frac{\mathrm{v}}{v}\,p$, the equation just found becomes

$$\frac{a\,\mathrm{E}\,\tau}{T} = \frac{\mathrm{V}-V}{V}P + \frac{\mathrm{v}-v}{v}p = (\mathrm{V}-V)\sqrt{G} + (\mathrm{v}-v)\sqrt{g}\;..\;(c)$$

supposing E the elasticity under which the specific gravities are taken.

Therefore, if we have the value of V say, equation (a) gives the value of v, and of course that of $V_1 = V + v$. Whence a being known, and E and T being given, we have the value of τ, the time of percolation.

Cor. 1. Employing the same notation as in *Cor.* 2 of the preceding Proposition, we have

$$\frac{a\mathrm{E}\tau}{T} = (\mathrm{V}-V)\sqrt{G} + (\mathrm{V}_1 - V_1)\sqrt{G_1} + (\mathrm{V}_2 - V_2)\sqrt{G_2} + \ldots (d)$$

to any number of terms.

Cor. 2. It appears by (c) that when V and $v = 0$, or the whole airs are exhausted, the time of percolation is the sum of the times due to each air. It is, however, not so when a given measure is let out of the mixed volume. The time, in this case, is less than it would be if the effused measure contained the whole of the gases in the reservoir. For the percolation being supposed to be so small that the gases can constantly adjust themselves, the lighter air will effuse faster, in proportion to its volume, than the heavier air, and the given measure will be made up quicker than, according to the specific gravities and proportional volumes of the two airs, it should.

Let us take an example or two.

1st. Suppose the reservoir contains half a volume of oxygen and the same of hydrogen, and that the whole, under a constant pressure, effuses out, $\mathbf{V}$ will then $= \mathrm{v} = \frac{1}{2}$, and V, v each equal 0, while $G = \sqrt{16} = 4$, and $g = 1$. So that the time will be $2\frac{1}{2}$.

2d. Let us suppose the same gases and the same proportions before the effusion is commenced, that is, equal measures of each, but that the reservoir contains six measures, out of which one, as before, is to be effused. Then we shall have

$$\mathrm{V} - V + \mathrm{v} - v = 1, \text{ and } \mathrm{V} = \mathrm{v} = 3.$$

Whence

$$V + v = 5, \text{ and by } (a)\ 27 = \frac{V^4}{v},$$

which gives

$$27\,V + V^4 = 135.$$

The root of this is $V = 2{\cdot}7828$ nearly. Hence by the equation $27\,v = V^4$, we find $v = 2{\cdot}2211$, and the quantities effused are ·2172 of oxygen, and

·7789 of hydrogen, making together ·9961, or nearly one measure of both. But the time is

$$\cdot 2172 \times 4 + \cdot 7789 = 1\cdot 6477,$$

instead of 2·5, as by the preceding example.

Though the above figures are evidently not exactly correct, owing to the root of the biquadratic not being found accurately, the difference between the two times is sufficient to show that the times of the effusion of an entire measure of mixture, and of a part equal to that measure from a much larger quantity of the same mixture, are very different, the effusion of the measure from the larger quantity being always done in less time.

We may here remark, that unless the whole volume is effused, we know not how any given proportions of gases can be run out of a large quantity, because the lighter gas will always percolate faster in volume than the heavier; and if the aperture is large, it may be irregularly so.

Mr. Graham, p. 81, has mentioned the result of an experiment which appears to be at variance with our conclusions. After remarking that certain other gases, in mixture, pass in times agreeing with their respective laws, he says, "but hydrogen and carburetted hydrogen lose more or less of their peculiar rate, and pass slower, when mixed with other gases. Thus the time of passage of a mixture of equal volumes of oxygen and hydrogen is 0·7255; instead of 0·6315," {·6573?} "the mean times of

1 and 0·2631," {1·0515 and ·2632?} "of those gases individually."

From this and another experiment, he considered that hydrogen in mixture exercises some peculiar retarding influence, which he proposes to turn to account in examining the purity of other gases. I confess my scepticism; but without the particulars of the experiments no one can say anything certain.

Proposition III.

Let Q be the primitive quantity of air in any volume, and q that at the end of any other time τ, G being its specific gravity; it is required to find the time of its running out.

Since the time in which proportional quantities or equal incremental volumes flow out, are (*Cor.* 1, Proposition 2), as the square roots of the specific gravities, $d\tau = -\sqrt{G}\,.\,\frac{dq}{q}$, and

$$\tau = A\sqrt{G}\,.\,\log\frac{Q}{q} = A\sqrt{G}.\log\frac{B}{b},$$

B, b being the heights of the barometers at the beginning and end; and A depends on the size of the aperture and other circumstances unconnected with the character of the gas.

If we have two gases, and the times be supposed equal, we shall have

$$\sqrt{G} \cdot \log \frac{B}{b} = \sqrt{G_1} \log \frac{B_1}{b_1}, \text{ whence } \frac{B}{b} = \left(\frac{B_1}{b_1}\right)^{\sqrt{\frac{G_1}{G}}}$$

from which, if B, b, B_1, b_1 are known, the ratio of G to G_1 may be found; or generally two of the three ratios being given, the third may be found.

Otherwise thus. Let N be the total number of particles contained in the volume V, and C the number that strike against the area a in unity time; then $aCd\tau = -dN$ the number that would flow out in the time $d\tau$. But, by Proposition II,

$$C \text{ is as } \frac{E}{T} \propto \frac{NT^2}{V\sqrt{G}} \times \frac{1}{T} \propto \frac{NT}{V\sqrt{G}}. \qquad \text{Therefore}$$

$$\frac{aNT}{V\sqrt{G}} d\tau = -dN, \text{ or } -ad\tau = \frac{V\sqrt{G}}{T} \cdot \frac{dN}{N},$$

That is,
$$\tau = \frac{V\sqrt{G}}{aT} \cdot \log \frac{B}{b} \quad . \quad . \quad (3),$$

where B, b, as before, are the barometric pressures at the beginning of the experiment and at the end of the time τ.

Cor. 1. According to our principles, a volume of air could not, under any circumstances of equal temperature, run out as fast as an equal one of less specific gravity. But by a change of temperature in one of them the percolation might be equally rapid. Thus, since by Proposition II the rate of percolation is as $\frac{\sqrt{G}}{T}$, when V is constant or the same, we shall have

$$\frac{\sqrt{G}}{T} = \frac{\sqrt{G_1}}{T_1}, \text{ or } \sqrt{G} : \sqrt{G_1} :: T : T_1.$$

So that if the temperatures are as the square roots of the specific gravities, the times of percolation will be equal.

Cor. 3. Hence there would be no means of making a mechanical mixture of two gases that should run out together. Be the proportions what they may, the lighter gas would filter out quicker than the other.

Scholium.

In all the preceding propositions the gases are understood to flow into empty space or infinite vacuums, and their temperatures to be uniform throughout them and the time of the efflux.

Gases, it seems, pass through the same apertures, either in the same or opposite directions, with very little interruption to each other, at least so we are taught by experiments. If then there were two equal volumes of any two airs—say oxygen and hydrogen for instance—and a small communication was opened between them, the hydrogen, which is about a 1-16th the specific gravity of the oxygen, would diffuse itself in a fourth of the time throughout the two spaces that the oxygen would. The effect of this would be, that the elastic force in the oxygen vessel would continue to rise after the communication was opened for some time, and that in the hydrogen to fall. For, as the hydrogen flows four times faster than the oxygen, it would come in and add to the elastic force of the oxygen much faster than the oxygen would flow into and raise the elasticity of the hydrogen. This would continue up to a certain point, and then the elasticity of the oxygen vessel would diminish and that of the hydrogen increase, until they became the same in both, and the gases were equally diffused throughout the two vessels.

This property has been illustrated by putting a wide-mouthed jar, containing common air—over the mouth of which is tightly tied a piece of sheet-caoutchouc—in a glass bell filled with hydrogen. The hydrogen percolates through the porous caoutchouc faster into the jar than the air escapes out, and so increases the elastic force in the vessel as to swell up and presently burst the caoutchouc covering.

This would happen in all purely mechanical cases, and where no superior chemical affinity or action takes place between one of the airs and the vessel or membrane dividing them. The lighter gas will always pass through the pores faster in quantity or bulk, though not faster as to number of particles; than the heavier; and the elasticity, therefore, will always increase on the side of the heavier gas. But the proceeding must be perfectly mechanical. There must be no chemical action at all; if there is, it may reverse the process, and the heavier gas flow faster into the lighter than the lighter into the heavier. Thus, if common air be confined in a bladder, and the bladder so filled be surrounded with carbonic acid gas, the latter, though heavier, will diffuse through the bladder faster than the former, though lighter; the consequence will be that the bladder will presently burst. The cause is a greater affinity between a wet membrane and carbonic acid, than between the membrane and air, by reason of which the mechanical law is vitiated. Generally speaking, wet membranes will allow the more condensible gases to pass with greater facility than they would the less condensible. It is, therefore, a matter of consequence in these experiments that the porous body be free from moisture, and everything which can affect the mechanical process of diffusion.

A new plan of measuring the specific gravity is that suggested by *Cor.* 1, namely, by finding the temperatures at which out of equal very small apertures equal volumes of the gases will flow in equal times. As these ranges of temperature are very great, with a slight difference in the specific gravities, it seems possible to determine the specific gravities to a great nicety. For instance, if the specific gravity of oxygen be 1·10563, that of air being 1, oxygen will diffuse or percolate as fast at 82°·7 Fahr., corresponding to the true temperature $\sqrt{1{\cdot}10563} = 1{\cdot}0515$ as air at 32° Fahr., corresponding to the true temperature

1·000, and as fast as nitrogen at 18°·7 Fahr., corresponding to the true temperature $\sqrt{\cdot 972} = \cdot 986$. The only and pressing difficulty of this is, to insure the same space of aperture at the two unequal temperatures.

Professor Graham's experiments illustrate some of the theorems of this Proposition. He took a long cylindrical tube, closed at one end, 9 inches long and 0·9 inch diameter, which he divided internally into 150 equal parts. To this tube he ground a stopper to fit the open end, and into this he fitted and luted a second tube 2 inches long, having a bore of 0·12 inch. This tube was bent, so that the outer part was at right angles, or nearly so, to the luted part.

The Professor filled this tube with various gases, and laid it horizontally on two small trestles, sometimes with the open aperture of the bent tube pointing upwards, and sometimes downwards, and after allowing the gas to diffuse out and the atmosphere in for 4, 10, or more hours, he measured the quantity of the gases then left in the tube.

The gases, it seems, left the tube exactly as our theorem shows they should. "It was determined," says he (Brande's Quarterly Journal of Science, v. xviii, p. 76), "with precision, in the case of olefiant gas, that that gas continues to leave a receiver, by diffusion, according to the same diminishing rate which holds in mechanical exhaustion by the air-pump," which is precisely the law of our theorem (3), p. 20.

As in experiments of this kind we suppose V, a, T

constant, we get

$$\log b = \log B - \frac{\sqrt{G}.(\log B - \log b)}{\sqrt{G}}$$

Instead of the hyperbolic logarithms as required in these investigations, common logarithms may be used, which renders the computations all the easier.

Professor Graham made two sets of observations, the one of four hours long and the other of ten, with several gases. The following table contains the quantities of gases he found at the two periods, compared with the above theorem:

Names of Gases.	Specific Gravity.	Quantity remaining after four hours. BY			Quantity remaining after ten hours. BY		
——	—	Obs.	Theor.	Diff.	Obs.	Theo.	Diff.
Hydrogen	1	28·1	30·9	+2·8	8·3	8·3	0
Carburetted hydrogen	8	85	86·6	+1·6	56	53·9	−2·1
Ammoniacal . . .	8½	89	88·0	−1·0	61	55·6	−5·4
Olefiant	14	99	99·3	+0·3	77·5	69·2	−8·3
Carbonic acid . .	22	104	108·2	+4·2	79·5	80·9	+1·4
Sulphurous ditto .	32	110	114·8	+4·8	81	89·9	+8·8
Chlorine	36	116	116·5	+0·5	91	92·6	+1·6

In the four hours' observations, the original quantity was 152 parts; in the latter, or ten hours, 150 parts.

One might see by this how well the theory and observations agree. In the ten hours' case the differences are wider and more irregular, but the character of the differences shows that they belong to the observations.

It should be observed, that when the two sets of observations are compared as to time, they do not agree. The ratio of the times is that of 10 to 5½ or 5·6, rather than that of 10 to 4. In other words, there is an inconsistency between these two sets of observations and the precise observation of Mr. Graham on olefiant gas. Possibly the inconsistency and the irregularity of the ten hour observations may be due to differences of temperature, which have considerable influence on the rates of diffusion. But so many circumstances come in to affect the observations, and the phenomena are so exposed to foreign influence, that these little irregularities may very reasonably be expected.

Probably if the aperture, which was 0·12 inch diameter, had been much less, the disagreement as to time would have been less. An eighth of an inch is rather too large an aperture for experiments of this kind, which require the conditions of equal density throughout to be rigidly preserved.

PROPOSITION IV.

By knowing the contents of the volume and time of flowing out, one may compute the sum total of the apertures through which the gas flows, or the amount of porosity.

For if V be the cubic content of the vessel, A the area of the pores, $\frac{V}{A}$ will be the length of a column of that area, which will flow out, if there be no interruption, just as fast as sound travels in the air over that length. At the temperature of 32° Fahr. sound travels 1090 feet per second in dry atmosphere, or $1{\cdot}090\ T$ at any other true temperature. Therefore, if τ be the time in seconds,

$$\tau = \frac{V}{1{\cdot}090\ TA} \text{ in the atmosphere.}$$

V being taken in cubic feet, and A in square feet. Hence

$$A = \frac{V}{1{\cdot}090\ T\tau}.$$

In any other gas τ must be changed into $\frac{\tau}{\sqrt{G}}$, and hence

$$A = \frac{V\sqrt{G}}{1{\cdot}090\ T\tau} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

in which τ is the number of seconds in the said air of its flowing out.

Scholium.

All the things in this Proposition are supposed to take place into an infinite vacuum through a thickless diaphragm, and the air is supposed to be kept constantly of the same density and temperature, which can only be done by a gradual contraction of the volume as the air flows out. If the volume remain constant, and it be true that airs do not interrupt each other in flowing simultaneously through the same apertures—which I think is doubtful, accurately speaking—the time will not agree with the present Proposition. In fact, the total time would be infinite, as the air by the preceding Proposition and the volume, never could be wholly emptied.

Professor Graham records some experiments, in which he says 60 cubic inches of air, effused through an aperture of "not more than 1-300th of an inch in diameter" in 1000 seconds into a nearly vacuous reservoir, kept so by constant pumping. If we take the second theorem before (4) and suppose V, T, and A to be expounded by inches, which would obviously not affect the value of τ, we shall, with $V = 60$, $\tau = 1000$, and $T = 1030$, which presumes the experiment to be made at about 61° Fahr., find the diameter of the aperture equal to 1-121·23dth part of an inch, which is a 1-200dth of an inch greater than the 1-300dth.

Now in measuring the diameter of the hole it is usually done by the diameter of the pin that drilled it; and we need not say that the pin is always less than the hole it makes. Our computation requires it to be only the 200dth of an inch larger diameter, which is probably within the limits of practical accuracy in such measurements.

PROPOSITION V. *Theorem.*

If a vessel containing a mechanical mixture of two gases be surrounded by an indefinite atmosphere of one of them, and diffusion take place through some small chinks or pores, and if the capacity of the containing vessel change so as always to maintain the same elasticity, there will be always the same number of particles in the vessel, provided the temperature and external pressure be invariable. That is, as many particles of the one gas will flow out in a given time as of the other will flow into the vessel.

Let A be the elasticity of one gas, and H that of the other within the vessel at any time, and E the sum of the two elasticities, or the elasticity of the mixture. That is,

$$E = A + H.$$

Now if A is the gas that is flowing in, and H that which is flowing out, and A change by the quantity a, H must also change by the same quantity a to maintain E invariable. Therefore one gas has increased as much in elasticity as the other has diminished. But the temperatures being the same, the numbers of particles that strike against equal spaces in a given time are (Proposition I, *Cor.* 1), directly as the elasticities, whatever be the character or specific gravity of the gases. The number of particles, therefore, of the internal airs A and H, which strike against the little apertures to go out in a given time, will be as $A + H$, and the number of the external air A which strike against the same little apertures to get into the vessel will be as E. But because $E = A + H$, the numbers that go out and come in during the same time will be equal. Therefore the number of particles contained in the vessel will always be the same, provided the temperature and elasticity remain the same, however much the gases within the vessel change. For instance, if the whole of the gas H percolate out, it will be replaced by exactly the same number of particles of the gas A. And this will hold good whether the airs pass in and out through the interstices or pores, perfectly independent of each other, or with any interirruption from their mutual collisions. For if one or more flowing-out particles come into collision and beat back one or more flowing-in particles, they themselves will be beaten back in precisely the same numbers, so as to leave the balance between the entering and going-out particles the same as if there was no interruption or collision.

Cor. 1. Hence if H was the sole gas occupying the vessel at first, the volume it then occupied, will be to the volume occupied by the air A after the whole of H has flowed out and is dissipated, as the square root of the specific gravity of A is to that of H; that is, the initial and final volumes of the airs are to each other reciprocally as the square roots of the specific gravities of the gases occupying them.

Cor. 2. No augmentation or diminution of temperature will take place in this process, nor of course any effect ensue from it, because whatever diminution of temperature may be supposed to occur in the flowing out of the particles, an equal augmentation must ensue by the flowing in of the same number.

Cor. 3. If the vessel contain two, three, or more airs mechanically mixed together, and diffusion take place in the way supposed in the Proposition, that is, by always keeping the air in the vessel at the same temperature and elasticity as the external air, all the internal airs will leak out and the same number of particles of the external air flow in, as was originally contained in the mixed airs. For the elasticities being always the same, there will every moment be the same number of particles flow in as flow out.

Cor. 4. This theorem and the corollaries, also afford means of determining whether an air be a mechanical mixture of other gases, or a strictly homogeneous gas. For if the air be homogeneous the initial and resulting volumes will be in the inverse subduplicate ratio of the specific gravities, supposing no chemical action to take place; and if they are not, the gases are not homogeneous, but a mechanical mixture of two or more of different densities.

Scholium.

Dalton, who first studied the curious phenomena of gases equally intermixing, without regard to situation or gravity, assumed for the explanation a singular hypothesis, namely, that one gas was a vacuum to another. Others have accounted for it on the hypothesis, that the particles of any one gas have a mutual repulsion for one another, but an attraction for those of all others.

Our theory explains the phenomenon very easily. In fact, as there are no laws or limits to the motions of the particles, the particles of two gases having a communication, must necessarily pass each other in their various motions, and will not be at rest until they shall have extended and diffused themselves uniformly throughout the spaces, as shown in Proposition VI, Book II, Section 2, p. 253.

Dœbereiner seems to have recorded the first observation upon what may be called the diffusion proper of gases. He observed that hydrogen confined in glass and other vessels over water, having cracks in them, always more or less escaped, and the water rose some inches in the course of a few hours. But when these vessels were covered with receivers, or filled with atmosphere, oxygen, or azote, he never observed any rise of water. Dœbereiner endeavoured to explain the phenomena by a capillary attraction of the fissure for hydrogen only. Professor Graham shows the error of the explanation. Dœbereiner indeed would have found it out himself, had he continued any one of the experiments long enough; for he would have discovered that the water actually sinks to its former level, the vessel being then entirely filled with common air and all the hydrogen gone. He would have seen that the same crack which let out the hydrogen also let in the air, and it would have taught him the fact, that the whole phenomenon is due to this one circumstance, namely, a slow and, as to volume, unequal percolation of air and hydrogen through the fissure, in which percolation the hydrogen escapes faster than the air enters.

By repeating Dœbereiner's experiments, and following them out with skill and perseverance, Mr. Graham has elicited the chief law which governs the phenomena. He has found that when the temperatures, elasticities, and apertures are the same, the volumes of different gases percolating in equal times, are in the inverse subduplicate ratio of the specific gravities. And so accurately does this law hold, that he conceives the specific gravities may be determined as correctly, or more so, by this method of diffusion than by the usual one of weighing. Certainly very small quantities may be operated on with nearly as much accuracy as large, which gives the method a great advantage. It is not, however, in the hands of every one that these experiments will succeed. Considerable care is requisite, and very delicate corrections; for unavoidable interferences have to be estimated and allowed for to make the experiments succeed, as any one will see who reads Professor Graham's paper in the twelfth volume of the 'Edinburgh Philosophical Transactions,' or in the second volume of the 'New and United Series of the Philosophical Magazine.'

The process he pursued, we have mentioned in the beginning, was by getting a glass tube about half an inch diameter, and 4, 5, or 6 inches long. One end was stopped up with a plug of plaster of Paris, from one to five or more tenths of an inch thick. The capacity of the tube he carefully graduated from the plug towards the other end, and then filling it with any gas, hydrogen for instance, he suspended it at the extremity of a balance having a counterpoising weight, with the open end of the diffusion tube downwards in water. The gas within began immediately to percolate and escape through the pores of the plaster, and, at the same time, the atmosphere to enter by the same pores. As the hydrogen escaped faster than the air entered, in respect of volume, the tube sank in the water so as constantly to keep the internal compound of hydrogen and air of the same elasticity. After some hours, according to the goodness of the plug and the character of the gas, the operation ceased. All the hydrogen had escaped, and its place was supplied with a less volume of pure air. This volume was to the one, at the beginning of the experiment, as the square root of the specific gravity of hydrogen to the square root of the specific gravity of air.

To obtain a larger volume and greater accuracy, the diffusion tube had sometimes a bulb blown near the plug, but the experiment was otherwise the same.

The process of escape and entry Mr. Graham calls the diffusion of the airs ; but if the elasticities within and without the tubes are different, he denominates the extra gas passing through from the side of the greater pressure, "gas mechanically forced through." We shall presently show that the one is just as much diffusion as the other.

The great difficulty in making experiments on diffusion is to obviate the effects of temperature. In the above method all effects of that kind were destroyed, because whatever temperature may be lost by the oozing out of the gas in the tube, as much exactly was restored by the oozing in of the same number of atmospheric particles. The temperature, therefore, was not affected. But where a less number comes in than goes out, or the contrary, the temperature may be so diminished or increased as very materially to interfere with the results.

We shall now extract some of the experiments of Mr. Graham.

A diffusion tube, 6 inches long, filled with hydrogen over mercury, and held fast, had the mercury rise 2 inches in the tube in 3 minutes, and within 20 minutes the whole of the hydrogen had escaped.

In a diffusion tube, 14 inches long, held fast, and filled with hydrogen over water, the water rose 6 or 8 inches in as many minutes. Shortly the water attained its maximum height, and then began to descend, and in the course of several hours it fell to the general level.

Experiments of this kind were uncertain, and the experimenter had therefore recourse to balancing the tube, so as to let it rise or fall, and always to keep the water at the same level within and without.

This is the true and legitimate way of doing it. In all his experiments Mr. Graham uses the specific gravities of the gases as given by the English, of which, however, it appears that the specific gravity of hydrogen is too high. Our English is ·0694, but Berzelius's ·06885. The former gives the diffusion 3·7947, the latter 3·8149, and by seven of Mr. Graham's experiments the diffusion volume is 3·869, but if we take the last five which he selects, it is 3·848. The extremes of the seven experiments are 3·774 and 3·900. We retain his own numbers, though they are evidently not the true ones.

The following Table exhibits the results of his experiments on nine gases compared with air as a standard.

Names of Gases.	Diffusion Volumes.		Specific Gravity.
	Expert.	Theory.	
Hydrogen	3·848	3·7947 or 3·815	·0694 ·06885
Carburetted ditto . .	1·344	1·3414	·555
Olefiant Gas . . .	1·0191	1·0140	·972
Carbonic Oxide . . .	1·0149	1·0140	·972
Nitrogen	1·0143	1·0140	·972
Oxygen	·9487	·9487	1·111
Sulphuretted Hydrogen .	·95	·9204	1·1805
Protoxide of Nitrogen .	·82	·8091	1·527
Carbonic Acid . . .	·812	·8091	1·527
Sulphurous Acid . .	·68	·6708	2·222

This diffusion power would give ·0675 for the specific gravity of hydrogen, and atmosphere would be 14·8 times heavier. If the specific gravity of hydrogen was ·068, the diffusion would be 3·8303, the same as Professor Graham gives it, and air would be 14¾ times heavier.

From the delicate nature of the experiments, Professor Graham thinks it hardly right to confide in his results beyond the second place of decimals. Be this as it may, the very close agreement of experiment and theory, leaves no doubt on the truth of the law, or of the homogeneity of the several gases.

We may now have the curiosity to examine whether we could by such experiments determine if the air be a chemical compound or a mechanical mixture. For example, if we multiply 1·0143 by 79, and ·9487 by 21, and add the two results 80·1297 and 19·9227 together, and then divide by 100, we should have the number of particles in unity volume of the mechanical mixture, or the diffusion volume of air, supposing it a mechanical mixture instead of a chemical. Performing the operation and we have 1·000524. So that if it was a mechanical mixture the index of diffusion would be so close to that of its being a chemical combination, that nothing could be determined from it.

One of the best proofs it appears to me of the air being a chemical compound, and not a mechanical mixture, is the fact of there being no duplication of sound. In foggy weather it is well known there are often two sounds, one transmitted more rapidly by the aqueous vapour, and the second by the atmosphere; and so it would be pretty generally if air was a mechanical mixture of two airs. The most usual sound would come by the nitrogen. A second feebler sound, however, would be transmitted by the oxygen, at a velocity about 6½ per cent. slower than the principal sound. But no such thing ever takes place. If it did it would be impossible at a distance to distinguish the notes, as we now can, of a fine piece of music. There would be complete confusion and destruction of all harmony with music heard a long way off.

PROPOSITION VI.

In the Graham experiment it is required to find the relation between the volume and the elasticity of the diffusing air, at any time of the experiment.

Let G_a represent the specific gravity of the atmosphere, G_h that of the air, h its elasticity at any time in the diffusion tube, and v the corresponding volume occupied by the mixture, the primitive volume being V. Then if E be the elasticity of the external air $\frac{vh\sqrt{G}}{E}$ is the number of particles of the air h in the volume, and $\frac{(E-h)\,v\sqrt{G}}{E}$ that of the atmosphere at the same time. But the sum of these two numbers is the same at all times, and equal to $V\sqrt{G_h}$, by the preceding Proposition. That is,

$$\frac{v}{E}\left(h\sqrt{G_h}+(E-h)\sqrt{G}\right)=V\sqrt{G_h}\;.\;.\;.\;(5).$$

Cor. 1. If for the sake of simplicity we put $G_a = 1$, we have, when $h = 0$,

$$v = V\sqrt{G_h}$$

the law of Graham, namely, $V : v :: 1 : \sqrt{G_h}$, or the initial and final velocities are reciprocally as the square roots of the specific gravities.

Cor. 2. The above theorem holds equally good if there were two, three, or more airs originally in the diffusion tube. For instance, if $h = p + q$, we shall have

$$\frac{v}{E}\left\{p\sqrt{G_p} + q\sqrt{G_q} + (E - p - q)\sqrt{G_a}\right\}$$

$$= V(\sqrt{G_p} + G_q),$$

as by following the same reasoning will appear.

Cor. 3. Putting $V = nv$ we have from (5)

$$\sqrt{\frac{G_h}{G_a}} = \frac{1 - \frac{h}{E}}{n - \frac{h}{E}}.$$

So that if G_h diminishes, h must increase, because n is supposed to be greater than unity. In other words, for the same proportional contraction of volume a less portion of a lighter gas goes out than of a heavier. Other considerations show the same thing. For if we conceive that one gas would be run all out with a contraction to a half the volume, and another or lighter to a fourth, it is obvious, when contracted to three fourths, that the one which would be exhausted at one half volume, must be more nearly spent than the other, which would not be exhausted until it reached the fourth of the original volume.

Cor. 4. If **D** denote Professor Graham's measure of diffusion power, that is $\sqrt{\frac{G_a}{G_h}}$, we shall have from (1)

$$1 + \left(\frac{E}{h} - 1\right) \mathbf{D} = \frac{EV}{hv} \quad . \quad . \quad . \quad (6),$$

which, when $h = 0$, becomes simply $\mathbf{D} = \frac{V}{v}$.

In this theorem

$$\frac{E}{h} = \frac{\mathbf{D} - 1}{\mathbf{D} - \frac{V}{v}}$$

PROPOSITION VII. PROBLEM.

Things being as in the last Proposition, it is required to find the time at which a gas diffusing, as in the Graham experiment, will descend from the elasticity E of the atmosphere to any other elasticity h.

If we suppose a to be the little quantity or number of particles that will diffuse out in unity time at the elasticity E, we shall have $\frac{ahdt}{E}$ for the number that will diffuse out in the time dt at the elasticity h. But during the time dt, the volume v contracts to $v - dv$. So that had no particles flowed out, the elasticity h would have become

$$\frac{vh}{v - dv};$$

from which taking h we have

$$\frac{hdv}{v} \quad . \quad . \quad . \qquad (7)$$

for the increment of elasticity, if there had been no loss or diffusion of particles in the time dt.

Again, let $A\sqrt{G_h}$ be the number of particles in unity volume at the elasticity E, and we have $\frac{vAh\sqrt{G_h}}{E}$ for the number in v volume at h elasticity. Whence, subtracting the quantity diffused out in the time dt, and, dividing by the said quantity, we shall have the factor of h, that would reduce it to the elasticity it would have if there was no contraction of volume, that is,

$$1 - \frac{adt}{Av\sqrt{G_h}}.$$

Multiplying by h, then diminishing by h, and lastly, adding $\frac{hdv}{v}$ found above, we shall have

$$h\left(\frac{dv}{v} - \frac{adt}{Av\sqrt{G_h}}\right) = dh,$$

the differential of the elasticity.

Whence, by (7),

$$dv - \frac{adt}{A\sqrt{G_h}} = \frac{EV\sqrt{G_h}\,.\,dh}{h\{h\sqrt{G_h} + (E-h)\sqrt{G_a}\}}.$$

Integrating and correcting both sides of this expression, we have

$$V - v + \frac{at}{A\sqrt{G_h}} =$$

$$\frac{V\sqrt{G_h}}{\sqrt{G_a}}\,.\log\left\{\left(\frac{E}{h}-1\right)\sqrt{\frac{G_a}{G_h}}+1\right\} \quad . \; . \; (8);$$

or, putting **D** for the diffusion power of the gas, that of the atmosphere being 1,

$$\frac{at\mathbf{D}}{A} = v - V + \frac{V}{\mathbf{D}}.\log\frac{V(\mathbf{D}-1)}{v\mathbf{D}-V} \quad . \; . \; (9).$$

Cor. 1. As **D** decreases $\mathbf{D}-1$ diminishes, but not so much as $v\mathbf{D}-V$, because v is supposed to be less than V. Therefore, as **D** diminishes, the logarithmic part increases; at the same time $\frac{V}{\mathbf{D}^2}$ and $\frac{v-V}{\mathbf{D}}$ rapidly increase. Consequently, V and v being constant, the time t rapidly increases as **D** diminishes, that is, as the diffusion gas is heavier. Or the time for a given contraction of volume is longer the heavier the gas in the diffusion tube. In other words, the air appears to make its way more slowly in against heavier than lighter gases.

Cor. 2. The law here developed of one gas diffusing, will hold where there are several in mechanical mixture, any one being selected at pleasure. For the investigation is carried on exclusively for that one, on the principle that the total number of particles in the diffusing tube is the same throughout the experiment.

Scholium.

Professor Graham's experiments afford no means of testing this theorem, as he has not given the times of the various stages of the experiments. In any future experiments undertaken on diffusion, it is to be hoped it will be done. He has stated that—" In a certain time, the same in all the experiments, a quantity of air entered," (we believe the diffusion tube), " by diffusion, which varied with the gas diffusing.

" In a given time,

With Chlorine	in the diffusion tube,		0·302	vol. air entered
„ Carbonic acid	„	„	0·623	
„ Hydrogen	„	„	1·277	

This is is conformable to *Cor.* 1, Proposition VII; but before we can submit these experiments to a comparison with our theory, we must know the details of the experiments. If the equal times are very small, so that the contraction of volume—if the experiment was made in the way we presume it to have been—was very small, the relation of the quantity of air entered would be very different to what it would be if the times were larger, and the contraction approached nearer the limits of either of them.

It is likewise necessary to know how the volume of air entered was measured, because in either case the gases must be diffusing out while the air is coming in, and the question, therefore, is how was the quantity of air that had entered determined.

According to another experiment, it would appear that hydrogen enters with equal rapidity against all gases in the diffusion tube, as will be seen by the following table, in which the barometer gage indicates the elasticity of the air or gas in the diffusion tube, the external pressure being 30 inches:

Barometer Gage. Inches.	Hydrogen against Hydrogen. Secs.	Hydrogen against Air. Secs.
15		
	215	217
14		
	245	236
13		

N.B. In the 'Philosophical Magazine,' from which we have copied this, there is evidently a mistake in the headings, which we have set right.

These experiments want repetition. They are inconsistent with what goes before. Thus the diffusion of hydrogen is 3·795 to atmosphere 1; but at 14 or 15 inches the diffusion of hydrogen out must be about half that of hydrogen in, which would give one inch in 215 seconds diffusion out, and 2 inches diffusion in. Therefore, while 2 inches of hydrogen are diffusing in at 30 inches, $\frac{1}{3795} = \cdot 2635$, or about ¼ inch of air should be diffusing out at 15 inches pressure; that is 1¾ inches should be the apparent infusion of hydrogen against air, in the time that 1 inch apparent infusion of hydrogen against hydrogen is made. I have, therefore, strong doubts that some effect of temperature, perhaps caused by the diffusion of a gas of one elasticity into that of another, was operating in these experiments.

If we take the numbers Professor Graham has found in another experiment, namely, 2·4 instead of 3·795, we shall still have 1·6 instead of 1.

Doubtless these experiments are extremely delicate to make, and the least irregularity of temperature will cause very material differences in the results. So that unless, as in the principal, or, as we have called it, Graham experiment, all chance of influence can be taken away, but little dependence can be placed on the results.

Let us take another case, in Professor Graham's own words:

" A small bell jar, with an opening at the top, was used, which opening was closed with a plug of Paris plaster, of half an inch in thickness, over which a brass cap and stopcock were fitted and cemented. This receiver was placed on the plate of an air-pump in perfect order, and exhausted. When the stopcock of the receiver was closed, nothing entered the exhausted receiver; but on opening it, either air entered, forcing its way through the pores of the stucco, or any gas which might be conducted to it, by means of a flexible tube from a proper magazine.

" The time was noted in which the mercury of the gage barometer, in communication with the receiver, fell two inches, always setting out with a gas of the tension of 1 inch mercury in the receiver, and stopping exactly when it attained a tension of 3 inches.

"Air entered, according to eight or ten experiments, made on different days, in within 10 seconds, more or less, of 10 minutes, and so whether the air was saturated with aqueous vapour or dry.

"The same volume of different gases entered in the times expressed in the following table, under the same pressure, or beginning at a pressure of 29 inches of mercury, and terminating with a pressure of 27 inches:

	m.	s.
Air, dry . . .	10	0
,, saturated with moisture at 60°	10	0
Carbonic acid . . .	10	0
Nitrogen	10	0
Oxygen	10	0
Carbonic oxide . . .	9	30
Olefiant gas	7	50
Coal gas	7	0
Hydrogen	4	0

"In repetitions of the experiments, the numbers oscillated 10 or 12, sometimes 20 seconds on either side of the numbers given in the table, from circumstances which could not easily be appreciated.****

"The greater the pressure the more rapidly are gases forced through the pores of the plug; but the quantity of the gas which penetrates in any given time is not exactly proportional to the pressure, at least in the case of air and hydrogen. By doubling the pressure, we do not quite so much as double the quantity of gas forced through; or a fixed quantity of gas does not enter in half time under a double pressure, as will be evident from the following table of observations. Pressure of atmosphere 30 inches." We shall presently see the true reason of it all.

In this table we have added the last column of ratios, and converted the minutes and seconds into seconds, for the easier obtaining of the ratios, and performing other arithmetical operations.

Height of Gage Barometer or excess of External Pressure.	Time of falling 1 inch by entry of		Time of Hydrogen being 1, time of Air is	Height of Gage Barometer or excess of External Pressure.	Time of falling 1 inch by entry of		Time of Hydrogen being 1, time of Air is
	Air.	Hydrogen.			Air.	Hydrogen.	
Inches.	Seconds	Seconds		Inches.	Seconds	Seconds	
29	300	110	2·73	18	432	170	2·54
28	323	120	2·69	17	455	190	2·40
27	315	115	2·74	16	490	210	2·33
26	330	115	2·87	15	520	215	2·42
25	335	120	2·79	14	550	245	2·24
24	345	122	2·83	13	595	250	2·38
23	360	133	2·71	12	660	255	2·58
22	365	130	2·82	11	700	270	2·59
21	390	155	2·52	10	750	320	2·35
20	395	150	2·63	9	855	460	1·88
19	423	160	2·65	8			

It appears from this table either that air diffuses into air too fast or hydrogen into hydrogen too slow for the inverse subduplicate rate of the specific gravities, inasmuch as the numbers representing air are too small compared with those representing hydrogen.

By the fourth column it also appears, that the rapidity of the diffusive property of air, relatively to hydrogen, increases the less the external excess, or the greater the number of particles of air, into which the diffusion takes place, or the more there has been diffused.

Again, by the second and third columns, it would seem that as the quantity in the recipient increases, or the difference between the internal and external pressures becomes less, the apparent diffusion of at least common air increases in celerity; that is, it is quicker than it should be if the ratio of diffusion followed rigidly the law of the excess of elasticity.

All these phenomena may be explained upon the fact that airs running into airs increase their temperatures, as Dalton found. For then the heavier the fluid the more particles go in for a given rise of the barometer, and the more the temperature is augmented.

PROPOSITION VIII. PROBLEM.

Two portions of the same kind of gas at equal temperatures being confined in two vessels V, v at different elasticities, and a communication being opened between *them, it is required to find the time at which they shall have any given difference of elasticity, supposing the temperatures of both to be kept the same throughout the experiment.*

By Proposition I the number of particles that will flow in a given time dt from the vessel V into v, supposing the elasticities to be E, e at that time, and that E is greater than e, is

$$\frac{E-e}{T}\,adt,$$

a being a constant, depending on the size of the communication. Whence N, n being the total numbers of particles in the volumes V, v respectively at that time,

$$dE = -E\,\frac{E-e}{TN}\,adt, \text{ and}$$

$$de = e\,\frac{E-e}{Tn}\,adt.$$

But by the laws of expansion and contraction of airs $N = \dfrac{VE\sqrt{G}}{T}$ and $n = \dfrac{ve\sqrt{G}}{T}$. Substituting these values for N and n, and subtracting one from the other, we get

$$dE - de = -\left(\frac{1}{V} + \frac{1}{v}\right)\frac{E-e}{\sqrt{G}}\,adt,$$

or
$$\frac{d\delta}{\delta} = -\left(\frac{1}{V} + \frac{1}{v}\right)\frac{adt}{\sqrt{G}},$$

where $$\delta = E - e,$$

and by integrating

$$\log \frac{\Delta}{\delta} = \left(\frac{1}{V} + \frac{1}{v}\right)\frac{at}{\sqrt{G}} \quad . \quad . \quad (10),$$

where Δ is the primitive difference of elasticities.

By knowing these three quantities, therefore, we determine a, and hence the relation between δ and t at any other time.

Cor. 1. Because the function, with respect to V and v is symmetrical, it matters not which vessel has the air of greatest elasticity, the times to effect given changes will be the same in either case.

Cor. 2. It appears that the less the specific gravity of the gas, the less, all other things alike, will be the time to effect a given change in the proportion of the square root of its specific gravity, or the inverse ratio of its diffusive power.

Cor. 3. If one of the volumes as V is infinite with regard to the other, the theorem becomes simply

$$\log \frac{\Delta}{\delta} = \frac{at}{v\sqrt{G}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11).$$

For all practical purposes this will be the same if one of the airs be kept, by any means, at a constant pressure.

PROPOSITION IX. PROBLEM.

The same things supposed, as in the preceding Proposition, but instead of portions of the same gas, let us *suppose one vessel V is filled with one gas of the specific gravity G, and another v with another of the specific gravity g. It is required to find their simultaneous relations corresponding to any time t, the primitive elasticities being E and e.*

We have already seen that if two gases are brought into communication, they will intermix and diffuse themselves uniformly throughout the two volumes. Whether the two gases dispose themselves, each as if the other was a vacuum, as Dalton supposed, or whether they constantly obstruct each other, is not known with certainty. The probability is, they do obstruct each other, but to what extent is yet to be determined by experiment. The way we propose to consider them is, to suppose, with Dalton, that they perform their diffusions independently one of the other.

On this assumption, and supposing the gases in each vessel are at the beginning of the experiment pure and unmixed with the other, we have by (10)

$$\log \frac{E}{\delta} = \left(\frac{1}{V} + \frac{1}{v}\right)\frac{at}{\sqrt{G}}$$
$$\text{and } \log \frac{e}{\delta_1} = \left(\frac{1}{V} + \frac{1}{v}\right)\frac{at}{\sqrt{g}} \qquad (12),$$

at the end of the time *t*. Hence we have

$$\sqrt{G}\,.\log\frac{E}{\delta} = \sqrt{g}\,.\log\frac{e}{\delta_1} \quad . \quad . \ (13),$$

an equation which furnishes the simultaneous relations of the elasticities of the two gases, assuming that the temperatures have been always the same in both vessels.

But if we now put E_1, E_{11} for the elasticities of the airs (originally in the vessel V) at the end of the time t in the vessels V, v respectively, and e_1, e_{11} the elasticities of the airs (originally in the vessel v) at the same period in the vessels v, V respectively, we shall have

$$E_1 + e_{11}, \text{ and } E_{11} + e_1$$

for the simultaneous elasticities of the mechanical mixtures of the two gases in the vessels V, v respectively at the end of the time t. Then because

$$E_1 - E_{11} = \delta, \text{ and } e_1 - e_{11} = \delta_1$$

there results

$$E_1 + e_{11} - (E_{11} + e_1) = \delta - \delta_1$$

the difference of the elasticities of the gaseous mixtures in the two vessels at the time t.

Putting p, q for the coefficients of t in the first and second equations of (12) and we have

$$\delta = \log^{-1}\{\log E - pt\} = E\,\varepsilon^{-pt}$$
$$\delta_1 = \log^{-1}\{\log e - qt\} = e\,\varepsilon^{-qt}$$

or $\Delta_1 = \delta - \delta_1 = E\varepsilon^{-pt} - e\,\varepsilon^{-qt}$ . . . (14)

an expression, giving the difference of the elasticities in the two vessels at the time t, assuming that the flowing of the airs from one vessel to the other does not affect the temperature, or that the effect on the temperature of that flowing is every moment neutralized, by the superior and rapid conducting powers of the vessels.

Cor. 1. If V is infinite, $p = \frac{a\mathrm{D}}{v}$, and $q = \frac{a\mathrm{D}_1}{v}$, D and D$_1$ being the diffusion powers. And if we suppose $E = e$, we have

$$\Delta_1 = E \,.\, \frac{\varepsilon^{qt} - \varepsilon^{pt}}{\varepsilon^{(p+q)t}} = E \,.\, \frac{\theta^{\mathrm{D}_1} - \theta^{\mathrm{D}}}{\theta^{\mathrm{D}+\mathrm{D}_1}} \quad . \; . \quad (15)$$

where $\varepsilon^{\frac{at}{v}} = \theta$.

Cor. 2. By putting in (14) $t + t_1$ for t, and subtracting one result from the other, we obtain the change in the difference of the elasticities in the time t_1.

Scholium.

As we have been under the necessity of assuming that the temperature is constant during the experiment, that is, that the diffusion of the air from one vessel to another has no effect on the temperature, our theorems could hardly be expected to agree with experiment, in which no account is taken of that increase of temperature so well known to ensue, nor any means provided to obviate it, yet it will be found in the following comparison with hydrogen, that the theory and experiment agree much better than could be expected.

Height of Gage Baro.	Difference of Logs. × 10,000.	Hydrogen Seconds.			Atmosphere Seconds.		
		Observ.	Theory.	Diff. Seconds	Observ.	Theory.	Diff. Seconds.
29							
	350·9	110	107	−3	300	299	−1
28							
	363·7	120	111	−9	323	310	−13
27							
	377·4	115	115	0	315	321	+6
26							
	392·2	115	120	+5	330	334	+4
25							
	408·2	120	125	+5	335	348	+13
24							
	425·6	122	130	+8	345	363	+18
23							
	444·5	133	136	+3	360	379	+19
22							
	465·2	130	142	+12	365	396	+31
21							
	487·9	155	149	−6	390	416	+26
20							
	512·9	150	156	+6	395	437	+42
19							
	540·7	160	165	+5	423	461	+38
18							
	571·6	170	174	+4	432	487	+55
17							
	606·2	190	185	−5	455	517	+62
16							
	645·4	210	197	−3	490	549	+59
15							
	689·9	215	210	−5	520	588	+68
14							
	741·1	245	226	−19	550	631	+81
13							
	800·4	250	244	−6	595	682	+87
12							
	870·1	255	265	+10	660	741	+81
11							
	953·1	270	291	+21	700	812	+112
10							
	1053·6	320	321	+1	750	898	+148
9							
	1177·8	460*	359	+101	855	1004	+149
8							

* I am inclined to think here are from 60 to 100 seconds wrong in this observation. It is probably 360.

In the first column of the preceding table are the heights of the gage barometers, or Δ and δ in (11); in the second the difference of the hyperbolical logs., agreeable to the said (11), multiplied by 10,000; in the other columns are the observations in the table at p. 47, compared with the said theorem (11).

The very near agreement of the numbers, and the alternation of the errors from + to —, for hydrogen between theory and observation, show that our principles successfully apply here. With atmospheric air, however, there is a constantly growing deficiency of the observations from theory, showing that some gradually increasing cause is operating to disturb the coincidence. It seems impossible that the more sluggish body and with larger particles should gain upon the more active at the time its sluggishness actually increases.

Now, if we consider that the number of particles would be in hydrogen only the $\frac{5}{19}$th of the number of atmosphere to depress the barometer an inch, it is quite evident that whatever effect the flowing in of hydrogen would have in raising the temperature, that atmosphere would have $\frac{19}{5} = 3{\cdot}8$ times as much. But where the excess of temperature is trifling, it may be, wholly or nearly, so absorbed by the sides of the containing vessel, as to produce no sensible effect, and very little disturb the agreement with theory on the supposition that there was no change of temperature. On the other hand, when the air is much heavier, and the particles more numerous, the sides may not be able to absorb it all as rapidly as it comes, and the consequence would be a considerable rise of temperature, an increase in the elasticity, and an apparent increase of rapidity in the entry of the air.

Moreover, at the plug or diaphragm where the diffusion takes place, the air which diffuses will cool it in proportion to the number of particles which percolate through. Therefore, the heavier air, by the longer continuance of the experiment, will be more cooled and become denser about the plug than the lighter, which by Proposition I, the elasticity being constant, will be actually the means of sending more of the heavier air in than of the lighter; that is, a greater number of particles in a given time.

These two causes, therefore, are quite competent to account for the apparent anomaly with the atmosphere. The heavier indeed the gas, the greater the anomaly ought to be. We may, therefore, consider ourselves fortunate in having so light and favorable a gas as hydrogen to compare theory with.

Was it not for the effect on temperature there is consequently little doubt but that the times of percolation for the two airs would follow more nearly the laws of diffusion than they do.

Mr. Graham has recorded some experiments on the flowing of gases through long fine-bored tubes, the phenomena of which he denominates "*transpiration*," but which we think are merely the common flowing affected by friction. He finds that the shorter the tubes are the nearer the times come to the rates of diffusion, but the longer the more they deviate from them. Hydrogen appears to be very much interrupted, but oxygen more than any gas. Olefiant gas, on the other hand, passes with great facility.

We see nothing in all this which may not be referred to some difference in the figure of the gaseous particles, which occasions one gas to have a greater friction than another, the effects of which diminish as the tubes are shortened.

SECT. II.

PROPAGATION OF SOUND THROUGH AERIFORM MEDIA.

VELOCITY OF SOUND.

PROPOSITION I.

If any disturbance or agitation be made in an air of uniform temperature, the effects of it will be propagated every way about with a uniform velocity.

Since the agitation can be communicated from part to part of the medium by the collisions only of the particles, it is clear that the celerity of communication will depend on the velocity of the particles. But if the temperature be uniform, this velocity will be the same, and therefore the rate by which the agitation is transmitted will be the same from the centre of agitation round throughout the medium.

Cor. 1. Because any excess of motion is communicated successively to all the surrounding particles, and divided among them, it follows that the agitation, whether great or small, is transmitted with very nearly the same rapidity to distant parts. If we suppose the excess of corpuscular velocity, occasioned by the agitation, to be communicated to a sphere of particles of a foot diameter, this excess, divided in a sphere of a 100 feet diameter, will be only a 1-1,000,000dth as great. And if, instead of its being divided among all the particles of the sphere, we conceive it to be divided merely among those of a spherical shell, it will then, at 50 feet distance, be a 1-10,000dth part as great. So that in whichever way we take it, any excess of motion will be so rapidly attenuated, as at the distance of only 50 feet to become altogether insensible. But if there were any irregularity in the rate of propagation of great and small sounds, it would only be at distances at which it could not, on account of their smallness, be appreciated.

Cor. 2. And if the particles of an air be indefinitely small, the celerity of transmission will remain the same, whatever be the elasticity of the medium, provided the temperature is the same. For the temperature being the same, the velocity of the particles on which the rate of transmission depends will be the same.

Cor. 3. What has been said of the uniformity of the propagation of sound in an open air, is likewise equally true in a hollow tube. For in all cases the mass of the tube is great in proportion to that of the air, for which reason, and because the air instantly communicates its excess of temperature to the sides of the tube, all effect from any excess of this kind is destroyed, perhaps more rapidly in a tube than in an open medium.

PROPOSITION II.

In any two homogeneous media the celerity of propagation is in a ratio compounded of the subduplicate of the elasticities directly, and the subduplicate of the densities inversely.

The elasticities are in a ratio compounded of the ratios of the numeratoms, temperatures, and corpuscular velocities. And since the ratio of the temperatures is equal to that compounded of the ratios of the velocities and masses of the particles, and the compound ratio of the masses and numeratoms makes the ratio of the densities, it follows that the ratio of the elasticities is equal to the ratio of the densities compounded with the duplicate ratio of the velocities. Therefore, the ratio of the velocities is equal to the subduplicate of that compounded of the elasticities directly and densities inversely.

Cor. 1. If V, v denote the velocities of propagation, D, d the densities, and E, e the elasticities of the media, then by this Proposition

$$V : v :: \sqrt{\frac{E}{D}} : \sqrt{\frac{e}{d}} \quad . \quad . \quad (1).$$

But if we call T, t the true temperatures of the media, and G, g their specific gravities, that is, their densities when the elasticities and temperatures are equal, then since in the same gas $D \propto \frac{GE}{T^2}$ and $d \propto \frac{ge}{t^2}$,

$$V : v :: T\sqrt{g} : t\sqrt{G} \quad . \quad . \quad (2).$$

Cor. 2. Hence if the specific gravities are equal, the velocities of propagation are as the temperatures; and if the temperatures are equal, they are in the subduplicate ratio inversely of the specific gravities.

PROPOSITION III. PROBLEM.

The specific gravity of the air and its elastic force being given, it is required to compute the velocity of sound.

At first sight one would imagine that the conditions given are insufficient for the solution of this problem. The size of the particles, the direction of their motions, or something of the kind, seems at an off-handed view to be indispensable; such at first I considered to be necessary. However, it happens from the concurrence of circumstances that nothing of the sort is wanting.

Our chief difficulty in this problem lies in discovering the manner in which the particles move among one another. This is indeed an impossibility; inasmuch as it is highly probable that the internal motions of the particles are at least partially subject to the influence of the figure of the containing vessel, as well as to its smoothness, and a variety of other circumstances which could not be reduced to calculation. All, therefore, that can be done is, to take such a view of their internal motions as should be equivalent to their mean state, and then compute the result. Now, if we imagine a mathematical cube to contain any given quantity of air, and conceive the particles so to move among themselves that the collisions are made simultaneously by sixes; that is—supposing the faces of the cube to point to the zenith, nadir, and four quarters of the world, and the particles to move at right angles to each other and the sides of the cube—if we suppose a particle from the zenith, another from the nadir, and one from each of the four quarters to meet all together, then to separate, each retracing its path until it meet another such a knot of particles, and so on throughout the air, a medium would be composed which might continue its existence and motions, if not otherwise disturbed, to an indefinite time. Each of the particles on this hypothesis would constantly run forward and backward in the same track, meeting at one extremity always one set of five particles, and at the other extremity always another set of five particles.

With this view, if N denote the numeratom, $\frac{1}{N^{1/3}}$ is the distance or length of a particle's path between the extremities, and $\frac{2}{N^{1/3}}$ the whole length it runs in going and returning. If, therefore, V be the velocity per second of a particle

$$V : 1 :: \frac{2}{\sqrt[3]{N}} : \frac{2}{\sqrt[3]{N}}$$

the time of one entire revolution. Hence $\frac{VN^{1/3}}{2}$ is the number of such revolutions per second.

Again, M being the mass of a particle, VM is its momentum. But because the particles are beaten back and made to retrace their paths, $2VM$ is the force of collision, or the intensity of a particle's direct stroke on the containing side.

Moreover, the space and number of particles being the same, each particle has the same number of points which it might traverse without encroaching on its neighbours, whatever be the figure of the containing vessel. And, therefore, if the velocity be the same, the number of collisions in an indefinitely long period, the mean length of each, and mean distance due to each would be the same, however the particles do really move among one another. But the intensity of collision of a particle on the side of the containing vessel would be very different in this mean state to what it would be in the hypothesis assumed. For as the particle may strike in any angle from 0 to 90°, the intermediate angle 45° is the mean angle in which it can strike the side. Therefore, instead of $2VM$, which we have found above for the direct force of collision, $\frac{2VM}{\sqrt{2}}$ is the force due to a particle in its mean state. Multiplying this by $\frac{VN^{1/3}}{2}$ gives the mean force of a particle per second; and this again by $N^{2/3}$, the number of particles which strike against unity area, gives

$$\frac{V^2MN}{\sqrt{2}}=\frac{V^2D}{\sqrt{2}},$$

the mean force per second of all the particles on unity of area, D being the density of the air.

But if g be the velocity of a falling body at the end of a second, and E the elasticity of the air, or weight it will support, Eg will also be the total action per second of the particles. Equating these values,

$$V=\sqrt{\frac{Eg\sqrt{2}}{D}} \quad . \quad . \quad . \quad (3),$$

the mean rectilinear velocity of the particles on the assumed hypothesis, supposing each particle to act on the side with its mean force; or it is the velocity according to Proposition I, with which sound or any disturbance is propagated every way about.

For the value of this expression we have, by M. Biot, $g = 9{\cdot}8088$ metres at Paris (Traité de Physique, t. ii, p. 15), or $= 9{\cdot}8088 \times 3{\cdot}28085 = 32{\cdot}18122$ English feet; and $E = bmD\,(1 + {\cdot}00375\,C)$, in which b is the height of the barometer, m the ratio of the weight of a given volume of mercury to that of an equal volume of air, and C the Centigrade temperature. When $C = 0$, and $b = {\cdot}76$ metres, M. Biot makes $m = 10463$. Therefore

$$V = \sqrt{10463 \times {\cdot}76 \times 9{\cdot}8088 \times \sqrt{2} \times (1 + {\cdot}00375\,C)}$$

$$= 332{\cdot}124\sqrt{1 + {\cdot}00375\,C} \text{ metres,}$$

$$\text{or} = 1089{\cdot}65\sqrt{\frac{f + 448}{480}} \text{ English feet} \quad . \quad . \quad (4)$$

f being Fahr. temperature, in a perfectly dry atmosphere, that is 332·124 metres, or 1089·65 English feet at the temperature of water freezing.

Cor. 1. A consequence results from the solution of this problem of so singular a nature, that it cannot be allowed to go unnoticed. Nothing in the course of the solution has been said or understood of the nature of heat. Now, by experiment, we know that E is always proportional to $D\sqrt{1 + {\cdot}00375\,C}$ or to $D\sqrt{448 + f}$; therefore by (3) V^2 is as $1 + {\cdot}00375\,C$ or $448 + f$. That is, the square of the velocity of the particles is proportional to the Centigrade or Fahr. temperature reckoned from a given point on the respective scales, below the freezing of water. Consequently, if the constitution of aeriform bodies be such as we have supposed, heat must consist in motion. Conversely, if heat does consist in motion, and if nature always keeps to the simplest principles, our physical constitution of airs must be the true one; for nothing can be simpler than that which deprives the particles of all properties, except one only, hardness, essential to their existence.

Cor. 2. Though g appears in (3), it has no influence in the value of V as to any change g may undergo from the variation of attraction; and V would therefore be the same at all points on the earth, and at all distances from it, as long as the temperature remains the same. For if the quantity of gas in a given space be the same, m will be constant, however much g varies; and that the quantity of gas may remain the same, b must be inversely as g, so that bg is invariable.

PROPOSITION IV. PROBLEM.

It is required to find the velocity with which sound is propagated in mixed media.

When mixed media are considered, it is not an easy matter to fix the manner in which sound is actually propagated. The solid parts of a gas are so insignificant compared with the space occupied, that two or more gases might be joint tenants of the same space, and yet their particles rarely come in contact, notwithstanding their constant motions to and fro. In such a case, the airs would be like separate occupants, their cotenancies would not interfere; and the sound would be propagated by each independently of the rest, with a velocity inversely in the order of its specific gravity. Such a case as this has actually occurred. Two reports of the same sound have been heard, the one preceding the other; the first, doubtless, was due to the vapour, and the second to the air.

In all cases of this kind, there would inevitably happen some interferences and anomalous collisions among the particles of the different gases; but if these be rare compared to the other, their effect would pass unnoticed.

However, a very different phenomenon to this may arise. If a gas of one specific gravity be mixed with another very unlike it, the unequal motions and extent of vibrations of the particles of the two bodies may create such confusion and disorder, at least for a time, as to prevent the regular transmission of pulses, and of course the propagation of sound. This has been observed by Mr. Leslie. On the introduction of hydrogen among air, under a glass in which a bell was placed, the sound was much enfeebled, and at last, I believe, quite destroyed.

Between these two cases, which are without the pale of calculation, we shall choose that which contemplates a mutual action among all the particles. That is, whatever be the sizes and consequently the spaces which the particles need to perform their excursions in, they are so arranged that each has that which it wants. Now, if we take a line of these particles of any length, it is evidently immaterial in whatever way or order we conceive them to be arranged. They may be either mixed without order, or all the particles of one size may follow in succession, and then those of another, and so on; still the time of propagation from one extremity of the line to the other will be the same in each case. On these principles the computation of the velocity of sound in the mixed media is easy.

Let G, g be the specific gravities of two gases, L, l their volumes when reduced to the same elasticity and temperature. Then if V be the velocity of sound in the first, $V\sqrt{\frac{G}{g}}$ will be the velocity in the second air; and $\frac{L}{V}$, $\frac{l\sqrt{g}}{V\sqrt{G}}$ the times in which sound in the airs respectively travels over distances proportional or equal to L, l. Hence

$$\left(\frac{L}{V}+\frac{l\sqrt{g}}{V\sqrt{G}}\right) : L+l :: 1 : \frac{L+l}{L\sqrt{G}+l\sqrt{g}}V\sqrt{G} \quad ..(5)$$

the velocity of sound per second in the mixed media. In the same way we might proceed if there be three or more airs.

Cor. 1. The usual way of estimating the quantities of these airs is by their elastic forces when occupying the same volume. But if E, e be the elasticities of the airs, we have $E \propto L$ and $e \propto l$; and therefore we have only to change L, l to E, e and

$$\frac{(E+e)V\sqrt{G}}{E\sqrt{G}+e\sqrt{g}} = V + \frac{\sqrt{G}-\sqrt{g}}{E\sqrt{G}+e\sqrt{g}}eV \quad . . (6)$$

is an expression of the velocity in terms of the elasticities.

Cor. 2. Should the two airs be one atmosphere and the other vapour, $\sqrt{\frac{G}{g}} = \sqrt{\frac{8}{5}} = 1{\cdot}2649$, according to the experiments of Gay Lussac, and therefore the part of (6) due to the effect of vapour is

$$\frac{\cdot 2649}{1{\cdot}2649\, E + e}\, eV.$$

If we put $\frac{E}{e}$ equal to 50, 100, 150, and 200, the coefficient of V becomes ·004123, ·002078, ·001389, and ·001043; or 4·11, 2·08, 1·39, and 1·04 feet per thousand. These numbers are very nearly proportional to $\frac{e}{E}$; so that if we multiply 208 by $\frac{e}{E}$ it will give very nearly the effect of vapour per thousand feet of velocity.

Or if we take the values of $\frac{e}{E}$ at ·03, ·02, ·01, ·005, the factors of V will be ·006138, ·004123, ·002078, ·001043; that is, 6·14, 4·12, 2·08, and 1·04 feet per thousand. This correction is always additive.

General Scholium.

I shall now compare the theory with some of the latest and best observations that we have.

The facts stated in Proposition I and its corollaries, that sound travels uniformly forward; that strong and weak sounds proceed with equal velocities; that the barometric pressure has no influence on the velocity, except inasmuch as it is caused by change of temperature; and that the rate of propagation is the same in a hollow tube as in the open air, abstracting any effects of friction, have long been admitted. The results deduced in Proposition II and its corollaries are likewise known to the scientific world, from the analysis of Newton and subsequent writers. But in fact all these circumstances are comprehended in Proposition III, the numerical values of which we have now to compare with phenomena.

Captain Parry has furnished us with a set of observations in the high north latitude of Port Bowen, 73° 13′ 39″ N., made at very low temperatures, and apparently with considerable attention to circumstances likely to influence the results. He has not included the state of the hygrometer: but at such low temperatures, it would not influence the experiments at a medium probably a fourth of a foot. Captain Parry's own account of the observations I have not seen, the following abstract is taken from 'Philosophical Transactions' for 1828, p. 97. The gun was pointed S. 71° 48′ E.

Date of Observ.	Barom. in Eng. inches.	Therm. Fahr.	Wind.		Weather.	No. Guns fired.	Velocity in Eng. feet.		Diffs.
							Observed.	Computed.	
1824.									
Nov. 24 .	29·841	— 7°	E.S.E.	Light	Overcast	5	1040·49	1044·5	+ 4·0
Dec. 8 .	29·561	— 9	N.N.E.	Squally	Very clear	6	1037·34	1042·0	+ 4·7
1825.									
Jan. 10 .	30·268	— 37	E.S.E.	Light	Clear	4	1029·01	1008·3	— 20·7
Feb. 7 .	29·647	— 24·5	N.E.	. .	Very clear	6	1020·99	1023·5	+ 2·5
,, 17 .	29·598	— 18	. .	Calm	Overcast	. .	1039·25	1031·3	— 7·9
,, 21 .	29·735	— 37·5	. .	. .	. .	. .	1010·28	1007·7	— 2.6
Mar. 2 .	30·398	— 38·5	Easterly	Light	. .	. .	1014.39	1006·5	— 7·9
,, 22 .	30·258	— 21·5	Westerly	. .	Clear & Fine	. .	1026·64	1027·2	+ 0·6
June 3 .	30·118	+ 33·5	Easterly	. .	Very clear	. .	1098·32	1091·3	— 7·0
,, 4 .	30·102	+ 35	S.E.	Strong	Clear	. .	1118·10	1093·1	— 25·0
M. temp. except Jan. 10 and June 4 . . .		— 15·31			Mean dif. excluding Jan. 10 and June 4				— 1·7

The observations for January 10th and June 4th are excluded in the mean; the latter on account of the strong wind, and the former on account of its great deviation from the other observations—particularly from that of March 2d, made at nearly the same temperature and states of the wind and air—which induces me to think there is some error in it. Probably it should have been 1009·01.

After excluding these observations, we see the error or difference between theory and observation amounts to only—1·7 feet at the mean temperature of —15°·31 Fahr.

The French Academicians in 1738 (Connaissance de Tems for 1825, p. 370), found the velocity of sound to be 1103·5 English feet at a Fahr. temperature of 42°·8. It does not appear that any allowance was made for vapour; and it seems that even the temperature was uncertain to about 1° Cent. Our theory gives a velocity of 1101·8 feet for the same temperature, differing 1·7 in defect.

In the 'Cambridge Transactions' for 1824, Dr. Gregory has published a series of experiments on the velocity of sound, distinguished for their uncommon accuracy, considering the shortness of the base lines employed. No hygrometer was here used. The mean of all the Doctor's experiments gave a velocity of 1107 feet at the temperature of 48°·6; and our theory 1108·3 or 1·3 in excess.

Dr. Moll has published some experiments, made in Holland in 1823, which are esteemed to be among the most accurate that we at present have. They appear in ‘Philosophical Transactions’ for 1824, pp. 424 et suiv. A long base was taken, and every care it seems to ensure accuracy by the exact reciprocity of the shots, and such attention to the state of the air as might enable us to compute the influence of the vapour. These experiments were made June 27th and 28th; but he mentions others made on the two preceding days, which he instances in proof of the errors which might be committed when the shots are not reciprocal or simultaneous. They differ from the preceding 10 feet, when reduced to the temperature of melting ice, and in a dry air; and they differ from each other 21 feet, though the temperatures do not by more than 7°·34 Fahr. He therefore neglects them. His reciprocal observations are the following.

Date. 1823.	Barom. metres.	Tens. Vap. metres.	Thermom.		Velocities.		Mean.
			Cent.	Fahr.	Observed.	Reduced to dry air at 32° Fahr.	
June 27th	·74475	·009253	11°·16	52·088	1116·032	1090·827	1089·744
28th	·74815	·008405	11·215	52·187	1113·669	1088·661	

In the first $\frac{e}{E} = \frac{\cdot 009253}{\cdot 74475} = \cdot 0124$ and in the second $\frac{e}{E} = \frac{\cdot 008405}{\cdot 74815} = \cdot 0112$. Hence the effect of vapour in the former is 2·6 feet, and in the second 2·3 feet; and the observed velocities therefore reduced to dry air 1113·4 and 1111·4 feet per second. Our velocities at the same temperature in dry air come out 1112·2 and 1112·3 feet per second, or 1·2 above and 0·9 beneath observation. The mean of these experiments therefore differs not two inches from theory.

The 'Connaissance de Tems' for 1825, pp. 361 et suiv. contains a detail of the experiments, made on the proposition of M. Laplace, by order of the Board of Longitude in Paris. A hygrometer was used in these experiments; but M. Arago, to whom the care of drawing up the memoir was intrusted, seems to have neglected its influence in computing the velocity of sound from the experiments. This defect is supplied by Laplace for one at least of the day's experiments, at p. 372 of the same 'Connaissance.' He makes it, ·571 metre or 1·9 English foot. If the same quantity be allowed for each day, the observations give 1116·5 feet at 60°·62 Fahr. and 1128·4 at 63°·95 in dry air, while the theory is 1121·6 and 1125·4, that is 5·1 too much and 3·0 too little. If all these observations be collected into one place, we shall be able to take a more correct view of the value of the observations and of the theory.

Names of the Observers.	Fahr. Temp. of Obs.	Velocities.		Diff.
		Observed.	Computed.	
Parry, means	− 15·31	1036·0	1034·3	− 1·7
Academicians	+ 42·8	1103·5	1101·8	− 1·7
Gregory . .	48·6	1107·0	1108·3	+ 1·3
Moll . . .	52·1	1113·4	1112·2	− 1·2
. . . . .	52·2	1111·4	1112·3	+ 0·9
French Board of Longitude	60·6	1116·5	1121·6	+ 5·1
	64·0	1128·4	1125·4	− 3·0
			Mean diff.	− ·04

Thus, the mean difference between theory and observations, when the latter are corrected for the hygrometer, as far as I am enabled to correct them, is less than half an inch. This difference is so minute, compared to what the observations can themselves be depended on, that had it been 50 times as great it would then have been within the limits of reasonable error. Laplace indeed, to humour his own computations, thinks that 10·4 feet are within fair limits; but the observations here given show very evidently the contrary. The experiments of the same observation may vary between the extremes to the amount of 10 or 12 feet, and yet be good; but the mean of a set of careful well-made experiments would never be allowed to wander to this extent.

This uncommonly close agreement of observation with theory, is a strong argument in favour of the atmosphere's being a homogeneous air, on the presumption of which the theory is founded.

I must here be permitted to make a few observations relative to the cause I apprehend of the above French Commission for determining the velocity of sound. The theory of elastic fluids, which forms the chief part of the present work, was published in the 'Annals of Philosophy,' printed the 1st of May, 1821 (a hint was given in the Number for July, 1816), and an account of it sent to M. Laplace in the June following. On the 10th of the succeeding September, that is, upwards of four months after the printing of my theory, and three after he was personally apprised of it, he read a paper on the same subject to the Academy of Sciences, which was printed in the 'Connaissance de Tems' for 1824. The 12th of December the same year, he had another paper on precisely the same subject read, and then extended it to finding the velocity of sound. This, with more of the kind, appeared in the 'Connaissance' for 1825. In the 'Phil. Mag'. for July and August, 1823, I examined these theories, and pointed out their defects and errors. But what I wish to call attention to is, that it was long after Laplace knew what I had done that he drew up his attempts to explain the phenomena of elastic fluids, and proposed to the Board of Longitude to re-examine the velocity of sound, with a view of testing his theories or of furnishing him with data to complete his suggestion relative to the evolution of heat, in the compression of the wave conveying the sound. His proposal was agreed to, and a commission charged with the execution of it, consisting of MM. Prony, Bouvard, Matthieu, and Arago; to which was added MM. Humboldt and Gay Lussac, besides assistance given by the Duke of Ragusa, Lieutenant-Colonel Laplace, Captain Pernitty, and M. Rieussec. In fact, every aid which could be, was praiseworthily afforded both by the government and individuals.

About the same time I applied to Mr. Canning, one of our ministers, on a subject of much greater importance, embracing not only the problem of sound but the cause of heat and gravitation. I wished to have some experiments which I had made and De Luc too, both agreeing, extended to much higher temperatures, and offered to pay all the expense, on this simple condition, that if they were successful I should be reimbursed. The same proposition was made to the Royal Society. However, the proposals, notwithstanding the importance of their object, did not succeed. From Mr. Canning I had a civil refusal, but from the Royal Society no answer at all to my application. But to return, Laplace, after every possible method of augmenting his result that he could suggest, has raised it to within about 10·4 feet of the last experiment but one of those, with which we have compared our theory; that is, about 16 feet less than the mean of the whole, or nearly 400 times farther from the mean than our result is. Considering the number of hypotheses Laplace has assumed, and the auxiliary experiments he has pressed into his service, 16 feet is a discrepancy of no trifling amount.

The time sound takes to travel over a given distance, either in a vertical, horizontal, or oblique direction, is evidently the same as that with which the air transmits heat—a problem that is very easy, when we know the rate at which the temperature decreases as we recede from the earth.

For instance, let y be the distance of any point in the air from any other point, x the altitude of the former, and A the angle which a line through the two points makes with the horizon; then $dy : dx :: 1 : \sin A$. But if v be the velocity of sound per second at the altitude x, and θ the time sound takes to travel from one station to the other, $v : 1 :: dy : d\theta = \frac{dy}{v} = \frac{dx}{v \sin A.}$ And in a future section we shall show that $x = a\,(F - f)$, and by (4) $v = b\sqrt{f + 448}$, in which $a = 326{\cdot}115687$ and $b = \frac{1089{\cdot}65}{\sqrt{480}} = 49{\cdot}735$. Substituting these values for x and v we have $\frac{dx}{v \sin A} = -\frac{adf}{\sin A\,.\,b\sqrt{f + 448.}}$ and

$$\theta = \int \frac{dx}{v \sin A} = \frac{2a}{b \sin A}\left\{\sqrt{F + 448} - \sqrt{f + 448}\right\} \;..(7)$$

an expression in terms of the temperatures and sine of the angle of inclination only, without either the distance or difference of elevation of the places. But as this rests on the presumption that the actual temperatures of the two places follow the law of diminution hereafter given, which only happens in the mean state of the air, and therefore but very rarely, it is probable that this theorem, which in a mathematical state of the atmosphere would be quite accurate, in practice would be found not so. We shall therefore endeavour to find out some other expression which, by including the temperatures at both stations, their difference of elevations and distance, shall include all interfering circumstances. This is very easy. For if in (7) we put for f its value $F - \frac{x}{a}$, it becomes

$$\theta = \frac{2aD}{bx}\left\{\sqrt{F + 448} \qquad \sqrt{F - \frac{x}{a} + 448}\right\}$$

in terms of the distance D, the difference of altitudes x, and the lower temperature F. And if in the same (7) we put for F its value $f+\frac{x}{a}$, we have

$$\theta=\frac{2aD}{bx}\left\{\sqrt{f+\frac{x}{a}+448}-\sqrt{f+448}\right\}$$

in which the upper temperature alone is concerned. Half the sum of these values of θ, supposing F and f the temperatures observed, will probably be nearer the truth than (7) could almost at any time be.

Probably the simple expression

$$\theta=\frac{2aD}{bx}\left\{\sqrt{f+\frac{x}{a}+448}-\sqrt{F-\frac{x}{a}+448}\right\}..(8),$$

the derivation of which from the last expressions or from (7) is obvious, may in practice be found equally correct.

If $x=0$ and $F=f$, all these values of θ will become vanishing fractions, and sink to $\frac{D}{v}$, the proper value of the time in a horizontal line.

When the places are in the same perpendicular, $x=D$ and there results

$$\theta=\frac{2a}{b}\left\{\sqrt{f+\frac{D}{a}+448}-\sqrt{F-\frac{D}{a}+448}\right\}(9).$$

If the time sound takes to travel from the bottom to the top of the atmosphere, and the contrary be required, (7) is a more convenient expression. By putting $f = -448$, it comes out

$$\theta = \frac{2a\sqrt{F+448}}{b} \quad . \quad . \quad . \quad . \quad . \quad . \quad (10),$$

which, when

$$F = 52^\circ, \text{ is } \frac{326{\cdot}1157 \times 2}{1089{\cdot}65} \times \sqrt{500 \times 480} = 4^m\,53^s{\cdot}3;$$

so that in $4^m\ 53^s$ sound will travel from the bottom to the top of the atmosphere, or from the top to the bottom, when the temperature at the surface is 52° Fahr. At a lower temperature it would take a less time, and at a higher temperature a greater.

VELOCITY IN DIFFERENT GASES.

When a single impulse is given to the air, it produces on the ear a sudden uncontinued sensation; the blow of a hammer on an anvil, for instance, or the collision of any two hard bodies, is heard only for an instant. But when a succession of these sounds is produced greater than about 16 in a second, it gives the sensation of a continued sound or tone. And if the rapidity of these sounds be augmented, without at all altering their separate intensity, the impression on the ear becomes more acute, and the tone is said to have a higher pitch. This pitch a good ear is capable of distinguishing with great nicety, even amidst sounds of very different natures and intensities. In wind instruments it is easily perceivable. The longer the tube is the graver is the note it produces; indeed, the graveness of a tone is directly proportional to the length of the tube, other things being alike. Hence, if the same instrument could be made to speak in airs of different specific gravities, the tones in airs of a less specific gravity would be more acute than in those of a greater, in the ratio of the velocities with which they convey sound. By the pitch of the tones, therefore, in different airs, the proportional velocities with which sounds do actually travel in these airs may be found.

On these principles MM. Chladni, Van Rees, Moll, &c., have attempted to estimate the velocity in several of the gases. In many they succeeded very well; but in hydrogen their velocities fell several hundred metres, from 300 to 500 short of what they ought to be by theory. Speculating on this, philosophers had already begun to consider hydrogen differently constituted from the other gases. However, the late accurate experiments of M. Dulong, in the 'Annales de Chimie,' t. xli, p. 150, have set this matter at rest, by showing that hydrogen is not an exception, but a beautiful illustration of the general law; and that the enormous differences in former experimenters must have arisen from their defective methods, or the impurities of their gas. Some of the other gases display much greater discrepancies. Carbonic acid gas, oxide of azote, and olefiant gas differ, the two former about seven metres, and the latter as much as 21, from what they ought to be according to their specific gravities. The specific gravity of the last, however, appears not to have been determined by experiment. It is difficult to assign the cause of such differences in gases, which, from their specific gravities, might be expected to be among the most accordant. The errors being all of the same kind would lead one to suppose, that some regular cause existed, and that this probably lay in the want of homogeneity in the gases. On examination, however, this is contrary to fact. For sound will travel faster in a mechanical mixture of two gases than in a homogeneous gas of the same specific gravity. Let e, e_1 and g, g_1 be the elasticities and specific gravities of two component airs, T the common temperature, and b the pressure under which the specific gravities were taken when T was unity. Then

$$(eg + e_1 g_1) \frac{1}{bT^2}$$

is the weight of unity volume of the compound air. And if V_1 be the velocity of sound in a homogeneous air of equal specific gravity,

$$V_1 = V \sqrt{\frac{EG}{eg + e_1 g_1}}$$

by (2), V, E, G being the velocity of sound, elasticity, and specific gravity of some standard air of comparison, as dry atmosphere for example, and

$E = e + e_1$. Again, supposing z the velocity in the compound gas, then by (6)

$$z = \frac{Ev\sqrt{g}}{e\sqrt{g} + e_1\sqrt{g_1}} = \frac{EV\sqrt{G}}{e\sqrt{g} + e_1\sqrt{g_1}}$$

in which v, being the velocity in the gas g, is therefore equal to $V\sqrt{\frac{G}{g}}$.

Dividing the value of z by that of V_1 we have

$$\frac{z}{V_1} = \frac{\sqrt{E(eg + e_1 g_1)}}{e\sqrt{g} + e_1\sqrt{g_1}},$$

where it will be found, after restoring the value of E, that the numerator exceeds the denominator, that is z exceeds V_1.

Hence it is highly probable that the deviations from theory in carbonic acid gas, oxide of azote, and olefiant gas, observed by M. Dulong, are the effects of accident, or the mode of experimenting, and are attributable to errors, the common attendants of such observations. Hydrogen, in the hands of Chladni, &c., differed in the same way.

Subjoined are the results, as given by M. Dulong, together with computations from the preceding formulæ. The specific gravities of the gases are those which M. Dulong took at the time of making the experiments.

Names of the Gases.	Specific Gravities.	Velocities in Metres.		Diff.
		Observed.	Computed.	
Common air .	1·	333·	332·	— ·9
Oxygen . . .	1·1026	317·17	316·29	— ·9
Hydrogen . .	·0688	1269·5	1266·21	— 3·3
Carb. acid gas .	1·524	261·6	269·03	+ 7·4
Carb. Oxide .	·974	337·4	336·53	— ·9
Protoxide of Azote	1·527	261·9	268·77	+ 6·9
Olefiant gas .	·981	314·	335·32	+ 21·3

Effect of Wind on the Velocity of Sound.

Whatever we have hitherto said, has been relative to airs supposed to be externally at rest. If the air itself be moving in space, the velocity in the direction of the air's motion will be greater by a quantity equal to that of the whole velocity of the air, and in an opposite direction less by an equal quantity. In any other direction it is easily found. For if V be the velocity in a still air, w that of the wind, and v that in the direction sought; and if z be the angle made at the origin of sound by the point from which the wind comes, and the point to which the sound is to be conveyed,

$$v = \sqrt{V^2 - w^2(1 - \cos z^2)} - w \cos z,$$

cos z being negative when $z > 90°$; that is, it follows the law of the composition of forces.

It had always been supposed that the wind had no effect on the velocity of sound; Dr. Gregory, I believe, was the first who found the contrary by experiment.

Remark.

It might here be observed that this section, being computed on the old coefficient 1·375 of Lussac and Dalton for the expansion of air, it might be supposed that the results would be thrown out by the new one, 1·3665, of MM. Regnault and Magnus. The difference, however, would not be more than 3 or 4 feet from water-freezing to water-boiling, gradually growing with the distance above and below 32° Fahr.; for, at 32° Fahr., the velocity is the same on both coefficients, the new coefficient giving less velocities than the old above 32° Fahr., and greater below. The influence, therefore, of the new coefficient is so small, as to be inappreciable at all experimental temperatures.

SECT. III.

INTENSITY OF SOUND.

PROPOSITION V.

Other things being alike, the intensity of sound produced by any cause is, in the same air, directly proportional to the density and true temperature of the air.

For, under the same circumstances, the number of particles disturbed by the cause producing the sound, will be directly proportional to the density of the air; and the force with which this number strikes the ear, will be as the number and temperature conjointly, that is, as the density and temperature.

Scholium.

I have not, in this Proposition, taken on myself to fix the exact manner in which sound is actually communicated to the ear, otherwise than what is generally admitted. After the example of others, I have assumed that the intensity of sound is directly as the quantity of air primitively set in motion; the introduction of the temperature is a condition arising immediately out of the fundamental principles of the nature of airs, agreeably to our theory. However, I will now briefly sketch the manner in which I imagine the aerial pulses may be generated and propagated. First, let us imagine a column of air in which the particles keep each other in equilibrio by strata of particles moving directly to and from the body creating the sound. Then if we imagine, while the particles of the contiguous stratum are from it, that the body advances a little towards them, on their return they will have a less distance to come back, and will thence return and meet the others before they have reached their former place of collision. These being thus driven back before their time, meet the particles of the next stratum earlier, and in a more advanced situation than they would, and so on throughout the medium. In this way a crowding of the particles, or what is understood by a condensation, takes place whenever the collisions occur earlier than they ought. And this condensation obviously travels forward in the medium at the same rate at which the particles themselves travel. We have taken this view on a small scale, merely to illustrate the matter in its simplest bearing. Instead of a mere meeting of the particles, and a reflecting of them before they arrive at the end of their path, if a number of them be driven forward among others farther off, then, by our principles of collision, the action of the particles among which they are driven, cannot repel them back again, but will merely prevent them from pressing forward farther. As this crowded state of things, however, cannot continue, the particles thus invaded will themselves, in turn, invade the space occupied by others still more advanced, and, if they can, likewise the space occupied by those to the right and left, which, in the same manner, will proceed to the next particles; and so on with these to others. A succession of crowdings or condensations will thus march forward from one extremity of the air to the other, spreading as it proceeds round the origin of motion, and presenting a force to any obstacle opposing it, by the mere crowding or condensation, without, as we have proved in Proposition I, any increase of temperature.

In this view it is clear that there is not an alternate condensation and rarefaction, but a crowding only from mere invasion, which travels uniformly forward, and spreads laterally, leaving no trace behind. Therefore the sensation of sound which it conveys, unless the cause be repeated, is single and momentary.

Hence if the sound be propagated through a tube, and the air has no room to dilate itself, the crowding of the particles will be the same in the whole of the progress throughout the tube, if its sections be of equal area; and consequently the intensity of sound will be the same. M. Biot has observed this. In the aqueduct at Paris, 951 metres in length, or 1040 yards, the slightest sound, even down to a whisper, was distinctly heard at the extremity; and airs on a flute played at one end of the aqueduct, arrived at the other most perfectly, in their true measure, and without the least confusion. (Traité de Physique, t. ii, p. 7.) But if the sections be unequal in different parts of the tube, and the aerial wave, as we suppose it to do, spreads equally about wherever it can, the intensity in different parts is inversely as the section.

On the same principles, in an open space, where the wave has free scope to dilate every way about, the intensity of sound will be inversely as the square of the distance from the origin of sound.

But though the crowding of the particles may dilate every way about, and the sound by this means be propagated in every direction, it is manifest that if sound be generated by a concussion in any one direction, that the greatest intensity will be felt in this direction, and that the intensity of sound will be less and less, as the angle from this direction increases. With guns, cannon, and speaking-trumpets, it is well known that the intensity of sound, in the direction to which they are pointed, much exceeds that in any other direction.

According to this theory of sound, the intensity will depend on two circumstances: the violence of the cause and the quantity of the air primitively agitated. If we imagine two bodies producing a sound by their collision to meet under precisely the same circumstances, but with double or triple the velocity, the sound generated would, I apprehend, be twice or thrice as great. And if, the velocity of meeting being the same, the bodies occupy twice or thrice the volume, under similar figures respectively, double or triple the air will be displaced; and therefore I conceive a double or triple sound will be generated. Hence, if a solid piece of wood, or other body of great thickness, be struck by a hammer, it must produce a less sound than a much thinner piece would which might vibrate with the blow. For in the first case that air alone is set in motion, and consequently creates sound, which is expressed from between the striking surfaces; but in the latter there is a quantity in addition set in motion, equal in area to the surface made to vibrate. Therefore, other things being alike, the greater the surface struck, if it vibrate at all by the blow, the more intense the sound. It is on this principle that a tuning-fork, struck and put on a sounding-board, develops a more intense tone than when held in the air; and that the sound of the Indian gong is so excessively loud.

Mr. Wheatstone has published some singular experiments relative to this subject and the communication of sound. He has found that the sound produced by a tuning-fork on a sounding-board, varies considerably with the angle which the plane of vibration of the fork makes with the plane of the board. The intensity is a maximum when these planes are at right angles, and a minimum when they are parallel. Mr. Wheatstone has also made several interesting experiments with rods bent in various angles; but the most singular of all this gentleman's experiments is that of communicating by means of a straight wire, or rod of wood, perpendicular to the sounding-board of any instrument, the vibrations to another sounding-board at any distance. By this means Mr. Wheatstone has been able to transmit a musical perform-ance from one apartment to another, with all its inflexions and shades of modulation. To appreciate, however, the bearing and value of these experiments, the original papers must be consulted. (Journal of the Royal Institution, 1831.)

PROPOSITION VI.

The same principles admitted, the intensity of sound, other things being alike, will be as the elasticity of the air and the square root of its specific gravity directly, and its temperature inversely.

For by (3), $V \propto \sqrt{\frac{E}{D}}$; and therefore $T^2N \propto V^2M^2N$ $\propto \frac{E^2M^2N}{D} \propto EM$; and consequently the intensity I of sound, which is as the temperature and number of the particles conjointly, is as $\frac{EM}{T}$. But T^2N being as $\frac{EM^2N}{D}$, if T and E be invariable, D is as the specific gravity; and hence

$$I \propto TN \propto \frac{EM}{T} \propto \frac{E\sqrt{G}}{T} \quad . \quad . \quad . \quad . \quad (11),$$

G being the specific gravity.

Cor. 1. If E and G be the same, the intensity is inversely as the temperature, or as the square root of the total Fahr. temperature from the absolute zero.

Zanetti has made an experiment corroborating this. He finds that sound is weaker in an open vessel in which air was heated, the elasticity being the same. (Hawksbee Experiments, vol. ii, p. 323.)

The lower the temperature is, therefore, the greater the intensity; and hence one reason that sound is better heard in the night than in the day, in winter than in summer. But in the night a very material reason is the cessation of that hum and buz, that variety of other sounds, which are incessantly occurring throughout the day. M. Biot, in the case of the aqueduct before cited, says, that for the success of his experiments it was absolutely necessary to have the greatest quietness, such, for example, as generally reigned an hour or two after midnight; for, in the day, he says the loudest cries could sometimes not be heard, so great was the effect of the disturbance of other external noises on the tube.

M. Humboldt imagines that the reason of the different audibility of sounds in the day and night, is owing chiefly to the inferior quantity of vapour in the latter, which, like the mixture of hydrogen with air, he conceives tends to deaden the sound. This may have some influence, but if it had the chief, sounds ought generally to become more and more audible as the night advances, and to be a maximum at sunrise when dew is most plentifully deposited, even though the temperature should be higher at this time than at sunset; a conclusion which, I believe, wants confirmation. However, as an increase of vapour commonly accompanies an increase of temperature, which diminishes sound, phenomena may appear to favour M. Humboldt's ideas.

Cor. 2. Other things alike, the greater the elasticity the greater the intensity, which furnishes one reason why bells and other instruments always give stronger sounds in low than in high situations. But in all these cases probably the sounds owe much of their superiority to the confinement of the situation, by which they are prevented from spreading and decaying so much as they would in an open situation.

Cor. 3. So if the elasticity and temperature be the same, the intensity of sound will be greater, the greater the specific gravity of the air in which it is made. In oxygen for instance, it ought to be greater than in common air, in the ratio of about $\sqrt{16}$ to $\sqrt{14{\cdot}4}$; in carbonic acid gas in the ratio of $\sqrt{22}$ to $\sqrt{14{\cdot}4}$; and in hydrogen less in proportion of 1 to $\sqrt{14{\cdot}4}$. Exact experiments on this subject are much wanting, but the general facts agree with the observations of Priestley. By inclosing in a receiver which could be filled with different gases a piece of clockwork, so contrived as to strike with a hammer at intervals, he found that oxygen gave sound rather louder than atmospheric air, carbonic acid gas still louder, but hydrogen exceedingly feeble. More recently, M. Perolle has made experiments of a similar kind on a variety of gases confirming those of Priestley.

Cor. 4. Because by (2), when the temperatures and elasticities are equal, the velocities are inversely as the square roots of the specific gravities, and, by the preceding corollary, the intensities are inversely as the velocities, it follows that the velocities in different airs might also be measured by the intensities of the sounds they emit. If we suppose the temperature alone constant, by the present Proposition

$$I : i :: E\sqrt{G} : e\sqrt{g}$$

and by (2) $\sqrt{G} : \sqrt{g} :: v : V.$

Therefore,

$$\left.\begin{array}{r} I : i :: Ev : eV \\ \text{or } V : v :: iE : Ie \end{array}\right\} \quad . \quad . \quad . \quad (12);$$

and consequently if the elasticities be so altered that the intensities shall be equal, and their equality can be well ascertained, the velocities will be directly measured by the elasticities of the airs. This would be a very simple experiment.

PROPOSITION VII.

The same things supposed, the same cause would generate sounds varying in intensity at different altitudes, in any atmosphere as the 11-12*th power of the elasticities or barometric pressures.*

By (25) Section 7 following, $E \propto T^{12}$, and consequently $I \propto \dfrac{E}{T} \propto E^{\frac{11}{12}}$ . . . . . (13).

Cor. 1. This Proposition supposes that the air is always in its mean state, or decreases in elasticity and temperature according to the theoretic laws in Section 6, and is free from all anomalies. Such a state, however, but rarely exists; and therefore it would probably be found much nearer the truth to introduce the temperature with the elasticity. If, therefore, we consider the numeratom to be, as it would be in the mean state of the air, as the 5-6th power of the elasticity, it will be

$$I \propto NT \propto \frac{NT^2}{T} \propto \frac{E}{T},$$

for different elevations in the same atmosphere.

Cor. 2. We see from this how it is that sound decreases so much in high situations; but that the decrease is less rapid than the elasticity decreases. Church bells, as we have already observed, are well known to sound much more feebly, and to be heard at a less distance, when situated at the top of a hill than in a valley. And on the Alps, Saussure found the report of a pistol to be no louder than that of a cracker below; but this case is not exactly parallel with the former.

PROPOSITION VIII.

A sound of a given intensity being generated at a given point in an air, varying in density and tempera-

ture, to find the intensity of the sound at any other point in the air.

Let S, N, T, I be the space through which the sound is diffused, the numeratom, the temperature, and the intensity of sound at the first place; and s, n, t, i the like things at the other point. Then if the numeratoms and temperatures were the same at both points, I would be to i as s to S. If the spaces and temperatures were the same, I would be to i as the effect on the particles of N individually multiplied by N, to the effect on the particles of n individually multiplied by n; that is, being merely a disturbance propagated from stratum to stratum, as $\frac{1}{N} \times N$ to $\frac{1}{n} \times n :: 1 : 1$. Lastly, were the spaces and numeratoms the same, I would be to i as T to t; for whatever be the cause of the difference of temperature, if we suppose the air to be in a state of equilibrious action, it is reasonable to believe that whatever diminishes the temperature will proportionably diminish the force of the invading particles from stratum to stratum. Therefore, if neither the spaces, numeratoms, nor temperatures be equal,

$$I : i :: sT : St \text{ and } i = \frac{St}{sT} I = \frac{StN}{s} A \quad . \quad . \quad (14),$$

in which A is a constant quantity.

This view is equivalent to a set of particles, in number proportional to the numeratom of the air at the said centre, being ejected from the centre producing the sound every way about within the limits of the space, and striking the ear individually with a force proportioned to the temperature of the air at the auditor.

Cor. 1. Hence, if the causes of sound were the same at two places elevated one above another in the atmosphere, the sensation of sound at the higher would be to that at the lower as $Nt : nT' :: B^{\frac{3}{4}} : b^{\frac{3}{4}}$, where B and b are the barometric heights at the lower and higher stations respectively. The intensity of sound, therefore, is much more feebly heard at the lower than at the higher station, the generating cause being the same. In other words, "sound ascends better than it descends." (Chladni, p. 277, Traité d'Acoustique.)

Cor. 2. If the generating causes be the same, the intensity of a sound, generated at any horizontal distance in the air, is to the intensity if generated at an equal distance below, as $n : N :: b^{\frac{5}{6}} : B^{\frac{5}{6}}$.

Cor. 3. If the sounds have freedom to spread in every direction, s will be as the square of the distance. Supposing, therefore, in the last *Cor.* the horizontal distance to be h, and the vertical v, the intensity in the horizontal line at the distance h will be to that in the vertical at the distance v as

$$\frac{b^{\frac{5}{6}}}{h^2} : \frac{B^{\frac{5}{6}}}{v^2} :: v^2 b^{\frac{5}{6}} : h^2 B^{\frac{5}{6}}.$$

Hence, if the intensities are equal, $v : h :: B^{\frac{5}{12}} : b^{\frac{5}{12}}$.

This is when the sounds are generated, the one beneath and the other at the horizontal level of the auditor.

Cor. 4. If the auditor be beneath one of the generating causes, and H be any horizontal distance from him at which an equal sound-producing cause operates, the intensity of the descending sound is to that of the horizontal sound as

$$\frac{STn}{V^2}A : \frac{STN}{H^2}A :: nH^2 : NV^2 :: b^{\frac{5}{6}}H^2 : B^{\frac{5}{6}}V^2.$$

And if the distances are sought at which the intensities are equal,

$$V : H :: b^{\frac{5}{12}} : B^{\frac{5}{12}}.$$

We have called V, v the vertical distances; but it is evident that the auditor need not be directly over or directly under the cause of sound. This position may be oblique as well as vertical; it is the difference of the barometric pressures, and not the direction which is of moment. However, the difference between the distances, when the intensities are equal, is the greatest if the auditor is directly above or beneath the cause of sound.

Cor. 5. When the intensities are equal, the vertical distance by *Cor.* 3 is greater than the horizontal if the sound ascends to the auditor; and the contrary by *Cor.* 4, if it descends.

Scholium.

By the help of *Cors.* 4 and 5 this part of the theory may be examined; for the ear may be depended on as to judging of equality of contemporary intensity pretty nearly. But I know of no experiments that have been made relative to the effect of vertical directions on sounds. All that we know is, that in lofty regions sounds are very feeble. In some cases the phenomena have been so striking as to induce parties to imagine that they have either lost, or suffered materially in their sense of hearing.

It has also been said that sounds ascend better than they descend, but no measures of the relative intensity have I believe been taken.

Wind, it is well known, has a remarkable effect on the intensity of sound, or distance at which any sound may be heard, though its effect on the velocity is scarcely appreciable. The same sound is often heard, and particularly in a strong wind, many times farther in the direction of the wind than in the opposite. This is obviously owing on one hand to the additional force given by the wind to the aerial wave in its impetus on the ear in the direction of the wind, and the diminution of its impetus in the opposite; and, on the other hand, to the condensation of the sound by the combined velocity of the wind and that of sound in the direction of the wind, and the extenuation of it by the same means in the opposite direction. These two causes, conspiring simultaneously to effect the same object, namely, to augment or diminish the intensity of sound, are the sole reason of that great difference in sounds which is observed on the windward and leeward sides of their origin.

We here assume that the whole body of air moves on together. If the air be subjected to any irregular motions, or anything interferes to disturb the pulses, both the nature and intensity of the sound will be altered, if not, perhaps destroyed.

Some experiments by MM. Bravais and Martius, detailed in tom. xiii, third series, 1845, of the 'Annales de la Chimie,' made in September 1844, show clearly that sounds generated high in the air are better heard below than those generated below are heard above. In their experiments on the velocities of ascending and descending sounds, they had one cannon on the top of Faulhorn, and the other at Tracht, 2079 metres lower level. They found that the mean ascending and descending velocities were the same, but in order to hear in the valley the cannon on the mountain, they were obliged to increase its charge of powder from 70 to 90 grammes; that in the valley being at 75.

SECT. IV.

QUALITY OF SOUND.

Philosophers have distinguished the impressions made by sounds into three kinds—intensity, quality, and pitch.

Intensity is synonymous, generally speaking, with what is understood by the term loudness. If, for example, a blow be struck with a hammer on an anvil, a sound is emitted of a certain intensity; and, by striking with a half, double, or triple the force, the sound is of a half, double, or triple the intensity, while the nature or quality of the sound is exactly the same. So likewise in any stringed instrument, if a given note be sounded with a greater or less force, the note varies merely in intensity, while the quality and pitch, at least, within certain limits, remains the same.

Quality is a property which produces an effect on the ear that enables it to judge in some degree of the cause of the sound, that is, whether it be the same as that of another sound or not. Thus the blow of a hammer on an anvil as before, is different from that of the same hammer on a block of wood; and the ear is able very distinctly to appreciate the difference, though perhaps the two sounds may be of precisely the same intensity, that is, may cease to be audible at the same distance. Again, any note on one instrument may, by management probably, be made to have the same intensity as the said note on another instrument, and yet the quality of the two notes is different, and easily distinguishable. In this case, too, the pitch will be the same, because the notes are the same.

By pitch is understood the number of pulses, or separate sounds which reach the ear from the sounding body in a given time. Recurring to our old instruments the hammer and anvil, if a succession of equal blows be struck, not less than about 16 per second, it will convey the sensation of a continued sound or tone. And if the number of them be increased to 32, they will make a tone of the same quality and intensity if the blows are equally violent, but of a double pitch, or what is called an octave higher. If there be 48 strokes in a second, the pitch is triple, or two octaves higher. In the same musical instrument the quality of all the notes, supposing the instrument perfect, is generally the same, but the pitch rises successively from the lowest note to the highest. Whether the intensity remains the same throughout the notes of the instrument is questionable, especially where the range is great. The occurrence of the same sound when very frequent, cannot fail to influence the impression of intensity; for the pulses succeed one other so rapidly, that the ear has not time to disengage itself from one impression before other pulses follow, and, riding in a manner one on another, cause the sensation of a stroke of greater force.

Pitch and intensity are hence effects the physical causes of which are obvious; but the cause of quality is much more intricate. We cannot refer it to the pulses merely as pulses; for, as pulses only, what idea can we form of them beyond intensity and frequency of recurrence, that is, beyond intensity and pitch? Breadth or direction of action on the ear can hardly, I presume, in the endless variety of sounds the ear is capable of appreciating, be called into our aid. To what, then, are we to attribute the quality? For my part, I can see no other cause than the manner in which the air is primitively set in motion. Let us, for instance, imagine an air to be internally in a state of quiescence, that is, let its particles perform their motions with regularity throughout the medium. In this state no sensation of sound can be conveyed to the ear; for the tympanum will itself remain undisturbed, on account of the equal and corresponding action of the external and internal air; but now, if a disturbance be made in the external air, such as that which causes sound, a sort of aerial wave will for a moment drive more particles against the external parts of the tympanum, and probably with very different modes of collision to what they had before. The excess in the number of particles striking the ear will produce the sensation of intensity, but the manner in which they strike will evidently have an effect on the nature of the sensation; and if in two sounds this sensation is unlike, we say the qualities differ. And since this manner of striking may be varied in an immense variety of ways, depending exclusively on the manner of the first disturbance, which must manifestly be transmitted from part to part of the medium, an immense variety of modifications to the sensation may be produced without in any ways affecting the number of particles which do actually create the impression of sound. Thus, without altering in the least the intensity, an unlimited change may be rung in the nature or quality of the sensation produced. But it is absolutely necessary there should be intensity, in order that the quality should be sensible. For, however much the particles may have their internal motions disturbed, if no pulse be generated no sensation of sound can be produced, because the individual actions of the particles cannot disturb the equilibrium so as to set the tympanum in vibration ; this can only be effected by the joint action of a body of them pressing unequally on the outside and inside. A beautiful illustration of this is given in the vibrations of two chords. If two chords similar, and of equal length, but not exactly in unison, be made to vibrate side by side, when the motions of the two chords are towards the same parts they emit a note of double intensity ; but, as the one which vibrates the faster gains on the other, that is, as their motions become less and less coincident, the sound decreases, until at length, when their motions are opposite, no sound is heard. At this period, though the air is equally disturbed as before, when the sounds were the strongest, yet as one string condenses it at the same time, and just as much as the other rarefies it, there can be no pulse, and therefore no sound. After this their motions concur more and more, and the tone gradually rises, until it reaches its former intensity.

Hence sound can have but two properties, intensity and quality. In fact, these are all that it has; for, as we have observed, pitch is merely an effect produced by the more or less frequent repetitions of the same sound.

From what is here said, whatever disturbs the air in the same way and produces a sound, makes one of the same quality. This is perfectly conformable to experience. Flutes have been made of several metals, woods, paper, glass, &c., and in all, when made alike, and the materials are too thick to be set in vibration by the action of the air, the sounds are precisely the same, proving incontestably that the quality of the sound is not due to the material, but to the manner in which the air is agitated. So, if a vibrating string be of any length, and stretched with any force, the quality of the sound remains the same, though both the pitch and intensity vary. In stringed instruments all the notes of any one are said to be of the same quality, notwithstanding the strings are of different thicknesses, and in some of different materials, and stretched with different forces. Thus it appears that the air is both the means and the carrier of sound. That is, it is not on the materials, which do actually occasion the sound, that even the quality of the sound depends, but on the kind of disturbance first given to the particles of air. Euler imagined that the quality arose from the sort of aerial wave that was engendered; but it is extremely difficult, as I have before said, to conceive how this can happen. Unequal thickness in the pulses would, I apprehend, be insufficient to explain that very great variety of sounds which we have. For the breadth of the pulse would, in the collision of bodies, be greatly affected by the velocity of their contact, it being greater the slower this velocity is; and therefore, if the quality of sound depends on the velocity of contact, the collision of two bodies, according as it is more or less smart, would yield a sound not of the same but of a different quality, which is contrary to experience. Were the quality to depend on any peculiarity in the form of the initial pulse, this would become more and more effaced as the distance from the origin of sound increased, by the different parts of the wave, running and blending themselves together. The quality of the sound would consequently be different at different distances, or different towards different parts, which is not found to be true.

M. Biot (Traité de Physique, tom. ii, p. 113,) concludes, from his experiments with wind instruments, that the quality of the sound depends on the materials of the tube. He asserts that a sound with a tube of glass is very easily distinguished from one with a tube of lead or wood; but afterwards conjectures that the difference may be owing to a feeble vibration of the materials affecting the issuing pulse. Happily the experiments of M. Savart have convinced us of the truth of M. Biot's conjecture, by showing that when the materials of the tubes are of sufficient thickness to resist the action of the vibrating column of air, the qualities of the tones are the same. They even go farther, and show that it is not the quality alone which is affected when the tubes are thin, but the pitch too of the tones.

Agreeably to our views, the quality of any sound will be the same at all distances. For, in whatever manner a stratum A is supposed to affect the internal motions of the particles of a stratum B, B will affect the motions of the particles of its neighbouring stratum C; and in the same way will C affect its neighbour D; and so on to the end. Now, as far as the ear is capable of judging, this is true, allowance being made for the enfeebled impression from the decay of intensity.

SECT. V.

GENERATION AND DESTRUCTION OF TEMPERATURE BY THE COMPRESSION AND RAREFACTION OF GASES.

Preliminary Observations.

Doctors Darwin and Cullen seem to have been the earliest who noticed that when air is compressed into a smaller space, heat is generated, and, on the contrary, lost when it is allowed to expand, by lessening the pressure, into a larger space. Our celebrated Dalton has, however, the credit of first attempting to measure the rise or fall of temperature due to the compression or rarefaction. According to him, 50° Fahr. of heat are generated when air is reduced to one half its volume; and as much lost by a corresponding expansion.

Leslie (Geometry, p. 495) says, that air compressed 1-90th part, will show a rise of temperature equal to 1° Fahr.

Dr. Ure has likewise, it seems, made some experiments of this kind, which confirm the results of Dalton. These experiments I have not seen.

The most perfect and useful set of experiments I have met with on this interesting subject, are those by Mr. Joule, of Manchester, printed in the 'Philosophical Magazine,' for May, 1845. These experiments are the only magazine from which we can get any useful stores, and it is with them we compare our deductions.

All gases, and airs of every kind, though not compressed to liquefaction, give out heat when they are reduced in volume, and the contrary. There is, however, a very remarkable difference in the results of compression of different gases. If chlorine, oxygen, and gases containing oxygen, are suddenly compressed to about a fiftieth of their volume, in a glass vessel containing dry tinder, the tinder will ignite; but the same phenomenon will not happen with other gases. They will all heat the tinder, but not ignite it. The reason assigned is, that they do not contain a combustible element.

There is great difficulty in ascertaining the exact rise or fall of temperature in compressions and rarefactions, by reason of the extreme mobility of the particles of the gases, and because of the very small quantity of matter in any gas to that of the surrounding bodies.

In order to understand the theory of the generation and destruction of temperature by compression and dilatation, let us conceive a quantity of air confined in a hollow parallelopiped. Then, while the containing vessel does not change its dimensions, the particles of its sides beat back the particles of the air with just the same force as that with which the particles of the gas strike it, that is, with a force which is measured by the common temperature of the two bodies.

If we now suppose one of the ends of the parallelopiped to commence moving inwards, its particles will have added to their own proper vibratory velocity the velocity of the moving end, and will therefore strike back the particles of the gas with a force greater than before. According to our principles of collision, by a force equal to the whole excess of motion arising from their velocity inwards. This, therefore, will cause the temperature of the air to rise, and produce the generation of heat.

On the other hand, if the end move outwards, the particles of the air striking against it will be struck back with a less than their normal force, and cause a fall of temperature and destruction of heat.

This is the simple theory of the phenomena which we shall now endeavour to evolve in mathematical laws.

PROPOSITION I. PROBLEM.

Supposing an air to be compressed by any means, and the additional heat be carried off as fast as generated by the surrounding bodies, it is required to find, on this assumption, the total quantity of heat generated by a given compression.

Let the elasticity of the gas occupying the space V at the temperature T, be E; and let e, t, v be its elasticity, temperature, and volume, occupied at any time τ from the commencement of the motion of the end or moving side.

Then if a be the area of the moving side, and s its velocity at the time τ, we have

$$-dv = asd\tau,$$

the motion being compressing.

Again, if C be the number of collisions of the particles against unity space in unity time of the moving end, when the gas occupies the space V at the temperature T and elasticity E, and c the corresponding number at the time τ under the circumstances, we have by (10) Book II, Section 2,

$\frac{c}{C} = \frac{eT}{Et} = \frac{tV}{Tv}$, since $\frac{e}{E} = \frac{t^2V}{T^2v}$, by (5) Book II, Sect. 2, putting $m = M$ and $\frac{N}{n} = \frac{v}{V}$.

But if h denotes the actual quantity of heat generated by the compression from the beginning during the time τ,

$$dh = asmcd\tau = -mcdv,$$

where m is the mass of a particle in the moving end. Substituting for c its value just found, and we have

$$dh = -mC\frac{tV}{Tv}dv \quad . \quad . \quad . \quad (1).$$

In this equation all the quantities are primitives except t and v. But by hypothesis $t = T$, and therefore

$$h = mCV \log\frac{V}{v} \quad . \quad . \quad . \quad . \quad (2).$$

Cor. 1. Hence the heat generated is as the logarithm of the compression, and has nothing to do with the rapidity of compression.

Cor. 2. It also appears that, for proportional compressions of the same volume, the heat generated is the same for all gases of the same temperature and elasticity, whatever be their specific gravity. For C is the same in all gases of equal temperature and elasticity.

Cor. 3. Again, the volumes being proportionally compressed, the heat generated is as the original volume of the air. So that a double volume would have a double heat generated, a triple a triple, and so on. Otherwise it is in the compound ratios of the logarithms of the compressions and original volumes.

Cor. 4. The primitive volumes and compressions being alike, the heats generated are as the numbers of primitive collisions against unity space; that is, as the elasticities directly and the true temperatures inversely.

Cor. 5. Lastly, the quantity of heat generated, all other things alike, is as the masses of the particles of the compressing end, or inversely as what are called their specific heats, or directly as their atomic weights. Though, therefore, it be immaterial what the nature of the gas is, that is inclosed and compressed, it is not so with the nature of the compressing end or side of the body.

Cor. 6. If **C** denote the number of collisions of the gas in unity time that would occur at $\mathbf{T} = 1$, $\mathbf{E} = 30$ the mean height of the barometer, and against unity space; and if **V** be the space it would then occupy we have $C = \frac{E\mathbf{C}}{30T} = \frac{T\mathbf{VC}}{V}$; so that (2) becomes

$$h = m\,\mathbf{C}T\mathbf{V}.\log\frac{V}{v} \quad . \quad . \quad . \quad (3).$$

The higher therefore the temperature is at which the compression begins, the greater the amount of heat communicated, all other things alike.

Cor. 7. From the absence of the area a of the compressing side in (3), it matters not by how small or large a space the compression is made.

Scholium.

Experiments are very deficient on this interesting branch of physics. Mr. Joule, of Manchester, appears to be the only philosopher, as previously observed, who has taken charge of the subject in the right direction. In his experiments in the 'Philosophical Magazine' for May 1845, he endeavoured to collect, by a very ingenious process, the amount of heat generated by compression and lost by rarefaction, agreeing with the principles on which the preceding Proposition and its corollaries are investigated. He forced, by means of a condensing pump, a quantity of air into a copper receiver immersed under a given quantity of water. The increase of temperature of the water was measured by a very delicate thermometer, allowances being made by express experiments to determine the effect of the friction in the working of the pump and apparatus.

He found that a compression of 21·654 atmospheres into the space of 1, or of 2956 cubic inches of air into the space of 136·5 cubic inches, gave a rise of temperature, on 45 lbs. 3 oz. of water besides 20½ lbs. of copper and 6 lbs. of tinned iron, equal to 13°·628 Fahr. per lb. This was the mean of six experiments, and as many for finding the amount of corrections.

By the mean of five other experiments for the heat, and as many for the correction, he found that 1433 cubic inches, compressed into the space of 136½, or a compression equal to 10·498 atmospheres into one, gave a rise of 5°·26 Fahr. per lb. of water employed.

These experiments are of so delicate a character that the corrections for the interfering effects of the friction, &c., of the apparatus exceed the whole net rise of temperature produced by the compression.

If we compare the two experiments with our theory, we have by (3) m, C, T the same in both, and V in one case equal to 21·654, and in the other to 10·498. These two numbers also express the ratios for these experiments of $\frac{V}{v}$. Therefore

$$21{\cdot}654 \times \log 21{\cdot}654 : 10{\cdot}498 \times \log 10{\cdot}498$$
$$:: 13^\circ{\cdot}628 : 5^\circ{\cdot}0516$$

for the rise of temperature on a lb. of water in the latter experiment. Mr. Joule found it to be 5°·26, or 0°·21 higher.

This very close agreement of theory and experiment, exposed to such disturbing causes as experiments of this nature are, appears to me to reflect great credit on the skill with which Mr. Joule conducted his experiments and estimated the sources of error.

There is a slight correction to be applied to our number 5°·0516, owing to the temperature of the air in the second experiment being higher than in the first, that is, 57°·5 Fahr., while the other was only 56°·2 Fahr. The difference, however, is only + 0°·0064, giving it 5°·057.

Mr. Joule's object is to prove that the heat generated by the compression and communicated to the water, is proportional to the mechanical force employed in the compression; and under the circumstances of the experiment he is perfectly right. For let l denote the whole labouring force expended from the commencement of the compression; then

$$dl = -aedx,$$

in which dx is the distance advanced by the compressing side, whose area is a. But $-adx = -dv$, and therefore

$$dl = -edv = -\frac{nt^2}{NT^2} Edv = -\frac{St^2E}{sT^2} dv \quad . \quad . \quad (4).$$

Dividing this equation by (1), and we obtain

$$\frac{dl}{dh} = \frac{tE}{TmC} \quad . \quad . \quad . \quad (5).$$

When the temperature is absorbed by the surrounding water as fast as generated, that is, when the water is in a large quantity and kept stirred, t continues very nearly equal to T, and the fraction (5) is a constant. The increments, therefore, of the labouring force and heat communicated, and consequently the total labouring or mechanical force and heat, have a fixed ratio.

Mr. Joule has computed that the mechanical force exhausted to raise 838 lbs. avoirdupois one foot high is sufficient, if employed in the compression of air, to generate heat enough to raise the temperature of water 1° Fahr. We shall presently return to this subject.

In another experiment Mr. Joule has immersed two receivers in the same vessel of water, one having 2956 cubic inches of air compressed into a space of 136½ cubic inches, and the other exhausted by means of an air-pump. A communication between the two being opened, and the water kept agitated, he found its temperature not at all affected, thereby proving that whatever heat was lost by the running out or expansion of the air in one receiver was gained by its running into and compression in the other.

Mr. Joule explains this by saying that no mechanical effect was produced, which is true enough, for mechanical power was lost in one and gained in the other. According to our views, a retreating column of air has precisely the same effect as a retreating side. If, then, we conceive a stratum of air to be retreating from one vessel into another and keeping up its own proper elasticity, just so much direct or onward motion as it has in its moving mass, is taken out of one vessel, and whether it goes into a vacuum direct against the sides of the empty vessel, or strikes against other particles of air, the same motion is communicated to the vessel or air into which it rushes, as is taken from that from which it comes. So that on the whole there is neither loss nor gain of heat.

Dalton's hypothesis of one gas being a vacuum to another falls before kindred experiments. For when a communication between vessels containing two gases is opened, if one gas be a vacuum to another, the loss of temperature generated by the expansion of the one into the double space, must be added to that generated by the expansion of the other; and therefore the united loss of temperature of the two would become very sensible. But no loss of temperature has been observed.

PROPOSITION II. PROBLEM.

It is required to compute, or to give an expression for, the exact quantity of heat communicated to a quantity of water under the circumstances of the preceding Proposition.

The same things being supposed as in the previous Proposition, let $s_,$ be the velocity of the advancing side at the end of the time τ, from the commencement of the compression, s being the velocity of a particle of the air at that time, and n the corresponding numeratom of the air. Then

$$s : 1' :: \frac{2}{n^{1/3}} : \frac{2}{sn^{1/3}}$$

the time of a revolution of a particle, or the time between its two consecutive collisions on the side. Therefore, $\frac{1}{2} sn^{1/3} d\tau$ is the number of times a particle strikes the area in $d\tau$ seconds.

Again $s_, m$ is the additional force with which a particle of the moving side would strike a particle of the air in the line of its motion. But since the particle of the air strikes the moving side in all angles from 0 to 90°, the angle 45° is the mean angle in which it strikes the side. In that angle then must the moving particle strike it. But that it should have s_1 direct velocity, it must have $s_1\sqrt{2}$ velocity in that angle of 45° to the direct motion. Therefore, the momentum communicated to each of the particles of the air in its mean angle of collision is, $ms_1\sqrt{2}$.

Hence the whole motion communicated in $d\tau$ time is

$$\sqrt{2}.ms_1\times\frac{1}{2}\ sn^{1/3}d\tau\times an^{2/3}=-\frac{mnsdv}{\sqrt{2}};$$

for the volume being diminishing, $-dv=as_1d\tau$.

But since $nv=\mathrm{N}$ is the total number of particles in the compressed medium, the whole motion communicated in $d\tau$ time is

$$-\frac{ms\mathrm{N}dv}{\sqrt{2}\,.\,v}$$

Now if t be the temperature generated by the compression during the time τ, dt will be that generated during the time $d\tau$. Supposing therefore N_1 to be the number of particles among which the heat is communicated, $g\mathrm{N}_1\,dt$ will be the total motion that would be generated during the time $d\tau$, g being the force of gravity $=32\frac{1}{6}$. So that

$$g\mathrm{N}_1dt=-\frac{ms\mathrm{N}dv}{\sqrt{2}\,.\,v}.$$

$$\text{Whence } dt = -\frac{m\ (T+t)\ \mathrm{N}dv}{\sqrt{2}\,.\,vm_{,}g\mathrm{N}_{1}} \quad . \quad . \quad (6),$$

supposing T the temperature at the commencement of the compression, and m_1 the mass of a particle of the air compressed.

If w be the number of particles in a pound of water and a those in a pound of air, m_1 a $=1$; and if it be atmosphere $\frac{3}{2}$ w (*Scholium* 1, Proposition V, Book II, Section 5, p. 349), or say nw $=$ a. So that if A be the weight of N particles $= m_1$ N of air, and W be the weight of water among which the heat is partly distributed, the above equation becomes

$$\frac{dt}{T+t} = -\frac{nmA}{\sqrt{2}\,.\,(W+nA)\,m_1 g}\cdot\frac{dv}{v} \quad . \quad . \quad (7).$$

And putting q for the coefficient of $-\frac{dv}{v}$ we have by integration

$$t = \left\{\frac{V^q}{v^q} - 1\right\} T \quad . \quad . \quad . \quad (8).$$

Cor. 1. It appears by the value of q in (7) that the greater the quantity of air A, the weight of water W being the same, the greater the rise of temperature for a proportional compression of the air.

Cor. 2. So the greater the mass m of a particle of the compressing body, the greater the rise of temperature.

Cor. 3. Again, the less the mass m_1 of the particles of the gas compressed, the greater the temperature generated, for the same weight of gas compressed.

Cor. 4. If **G** denote the specific gravity of the gas at the temperature $\mathrm{T}=1000$ and elasticity $\mathrm{B}=30$, the specific gravity of the atmosphere being unity, and **V** be supposed to be its volume under these circumstances, then is $V=\frac{A}{1}=\frac{A_1}{\mathrm{G}}$, in which A_1 is the weight of the volume V of any gas,

and
$$A=\mathrm{G}\mathbf{V}=\mathrm{G}V\frac{E\mathrm{T}^2}{\mathrm{B}T^2}.$$

Substituting, therefore, for A in the integral of (7), its value just found, and it becomes

$$\log\frac{T+t}{T}=\frac{nmVE\mathrm{T}^2\mathrm{G}}{\sqrt{2}.(W\mathrm{B}T^2+nVE\mathrm{T}^2\mathrm{G})m_1g}\log\frac{V}{v}\ \ldots(9)$$

a general theorem, whatever be the gas, taking n for the numeratom of a given weight of the gas, that of the same weight of water being 1.

When $\mathrm{G}=1$, and $n=1\frac{1}{2}$, the expression represents common air.

To render the formula homogeneous, W should be taken as a volume and multiplied by 814, the unity of W and V being the same, that is, a cubic inch or cubic foot.

Cor. 5. If $W=0$, the integral of (7) becomes

$$\log\frac{T+t}{T}=\frac{m}{m_1g\sqrt{2}}.\log\frac{V}{v}\quad\ldots(10).$$

Scholium.

Let us now see how far the theorem deduced agrees with actual experiment. Before Mr. Joule's experiments already alluded to, we had none that I know of which directly collect and measure the quantity of heat generated by the compression of the air. Dalton's experiments in the second part of vol. v, 'Manchester Memoirs,' do not do this. They endeavour to ascertain the amount by an estimate from correlative experiments. But these experiments, as Dalton himself perceived, were exposed to considerable errors. His method was by exhausting a receiver, with a delicate thermometer within it, and observing, during a very small portion of time, 3½ seconds, the rise of temperature on suffering the air to flow in through an aperture. He then heated a thermometer to 50° above the atmosphere, and observed that it fell the very same 1° in the same 3½ seconds of time. From this he inferred that the air by compression had been heated 50°, but that it had been rendered insensible, except in a small degree, by the small quantity of matter in the 120 cubic inches of air he had experimented with, and by the rapidity with which the sides of the receiver had abstracted it.

We scarcely need observe, that these indirect estimates, however ingenious, can rarely be depended on, and are never satisfactory, not even in the hands of such men as Dalton. Of their uncertainty we cannot quote a higher authority than this celebrated philosopher himself. He found that the size of the bulb and position of the thermometer had a material influence. With two thermometers, similarly placed, the one whose bulb was ·35 inch and the other ·65 inch diameter, the same experiment gave 2°·8 and 2°·2. Again, the same thermometer, 1st, with its bulb in the centre of the receiver, 2d, on the wet leather of the plate (of the air-pump), or 3d, against the side of the receiver, gave the following results:

1st case, reduced by exhaustion	2°·45
2d ,,	2·15
3d ,,	1·2
1st case, raised by restoring air	4·05
2d ,,	2·25
3d ,,	2·8

The difference in the diameter of the bulbs of the thermometers would make a difference in an estimate of 50° of 13°·6; for 2·2 : 2·8 :: 50° : 63°·6. However skilful the operator, therefore, there is no certainty in estimates from such experiments.

Besides this, there are causes interfering, in the way in which the experiments were made, that render conclusions from comparisons very questionable. We will mention one, the effect of radiation. The object is to determine the temperature of the gas. Now in the receiver the whole elevation of the thermometer is only between 2° and 3°, but in the air it is 50°. The radiation from the 50° excess, and the disturbance on that account, is much greater than in the former case of only 2° or 3° excess.

Again, the excess of heat in 120 cubic inches, or 37·2 grains of air, would have a much less effect, spread over that and the matter of the thermometer, which was probably a great deal more than that of the air, than the entire atmosphere at the same given difference of temperature would have. So that had it been possible to use thermometers with an infinitely small quantity of matter, the apparent rise or loss would have been much greater, and therefore the estimate of the effect would be greater, though, in point of fact, the effect would be the same.

These considerations would go to show that the heat given out by the compressed air was greater than the estimate makes it. But if we consider that glass is a very slow conductor of heat, and that the time of the experiment is very short, it is probable that the film of glass of the balloon affected by the heat is very thin, and that the quantity of matter among which, with the gas, the heat is diffused, does not many times—perhaps about four or five—exceed that of the gas. Therefore, if the whole difference in the air could be measured by an infinitely small thermometer, it would not be more than four or five times that which it actually appears to be. This, of course, is a mere guess, but it seems to show that the estimate, from the cooling of the thermometer, is a great deal too high. All circumstances, therefore, concur to make Dalton's estimate too high, and unquestionably very uncertain.

The method pursued by Mr. Joule, namely, by working all the apparatus under water, and then deducting the effects of friction, is the way to come at the truth, provided the effects of friction and stirring can be truly valued. They are great in respect of the effects of compression, and therefore may throw some uncertainty over the results; but it is possible, notwithstanding, for the true measure of the effect to be ascertained by repeating and taking a mean of several experiments, as he has done.

By one experiment, Mr. Joule pumped 2956 cubic inches of dry atmosphere into a space of 136½ cubic inches, or 21·654 measures into the space of one. The quantity of matter throughout which the heat was diffused, was

Water	45·1875 lbs.
20½ lbs. Brass, &c. equal to	1·9454 lbs. water
6 lbs. Iron . . .	·6600
Total equal to	47·7929 lbs. water.

Now $A = \frac{2956 \times 31}{7000} = {\cdot}1309$ lb. weight, and W, as we have found, equals 47·7929. In air $n = \frac{3}{2}$. Supposing, therefore, $m = m_1$ we have

$$q = \frac{3 \times {\cdot}1309}{\sqrt{2}\,.\,(47{\cdot}7929 \times 2 + 3 \times {\cdot}1309) \times 32} = \frac{1}{11060{\cdot}6}.$$

The mean temperature of the room was 56°·2 Fahr. which gives $T = 1025$. Hence

$$t = (21{\cdot}654^{\frac{1}{11060{\cdot}6}} - 1) \times 1025 = {\cdot}285$$

the exact quantity found by experiment by Mr. Joule in Fahr. degrees. But since, in these small quantities, the true and Fahr. temperatures differ but very little, it may be taken for the one or the other. Multiplying it by 47·7929 and it becomes 13°·621 for the increase of temperature to 1 lb. of water, according to Mr. Joule's views. He computes it at 13°·628. The difference is very small.

Another experiment was the compressing of 1433 cubic inches into the same space, 136½ cubic inches, or 10·498 into one, at a temperature of 57°·5, or true temperature 1026. We take it for granted, that the water and materials over which the temperature is diffused are the same. Hence, following the same process, we find the rise of true temperature to be ·0105678; and per lb. of water 5°·041. Mr. Joule, from his experiment, computes it to be 5°·26, pretty close to ours.

The theorems, therefore, that we have investigated, agree in quantity as well as law with the experiments.

PROPOSITION III. PROBLEM.

Let it be required to investigate the amount of labouring force expended in the compression of the air for a given rise of temperature; or let it be required to investigate the relation between the force employed in compressing the air and the heat generated.

The labouring force, as said in our definition of it, is the sum of all the forces exerted at every point of the distance gone over to overcome the resistances. It has no regard whatever to time, but merely to the work done. Thus, whether a given load is carried over a given distance in a minute or a year it matters not, the labouring force is the same. For a uniform resistance, therefore, the labouring force is the distance multiplied into the resistance or force employed, no matter whether the work is accomplished in an hour or a month.

Hence, if L denote the labouring force to effect a given compression of the air, and E be the elasticity at any point

$$dL = aEdx = - Edv\,;$$

in which dx is the distance moved over, E the elasticity of the gas, and a the area on which the pressure is applied as before. But by (3) section 2, Book III, p. 63.

$$s = V\sqrt{2} = \sqrt{\frac{2Eg}{D}}.$$

For in the mean motion of the particles the direction is at an angle of 45° to the propagation of sound, and therefore the velocity of sound V must be multiplied by $\sqrt{2}$ to give the celerity of the particles of the air. Consequently

$$E = \frac{s^2 D}{2g} = \frac{s^2 m_1 n}{2g} = \frac{(T+t)^2 n}{2m_1 g}.$$

Hence

$$dL = -\frac{(T+t)^2 n}{2m_1 g}\,.\,dv = -\frac{(T+t)^2 \mathrm{N}}{2m_1 g}\,.\,\frac{dv}{v} \quad . \; . \; . \; (11).$$

Dividing this by (6) and we have

$$\frac{dL}{dt} = \frac{(T+t)N_1}{2\sqrt{2} \cdot m} \quad . \quad . \quad . \quad . \quad . \quad . \quad (12),$$

and

$$L = \frac{N_1 t}{2\sqrt{2} \cdot m} \cdot (2T+t) \quad . \quad . \quad . \quad (13),$$

for the relation between the labouring force and the rise of temperature, induced by the compression among the particles N_1.

Cor. 1. Hence, if the particles N_1 remain the same, and t be very small, the labouring force is as the temperature generated, as Mr. Joule has proved experimentally.

Cor. 2. If the number of particles be not the same, the labouring force, when the rise of temperature is very small, is in a compound ratio of that rise and the number of particles among which the heat is distributed. In other words, the labouring force is as the whole heat generated.

Cor. 3. The same things being supposed of the smallness of t in respect of T, the labouring force is, all other things the same, as the true temperature of the air compressed. So that the labouring force is conjointly as the heat generated and the temperature at which the bodies were at the commencement of the experiment.

Scholium.

According to the theorems investigated in this section, the laws and measures are the same, whether it be heat generated by compression or destroyed by rarefaction. They are investigated for a compression, but, by merely changing the sign of t the temperature, they do for expansion.

By this Proposition and its corollaries the measure of the heat generated or destroyed is really the labouring force. If there is no labouring force expended there is really no change of temperature, or rather no generation or destruction of heat. Air, therefore, let out of one vessel in which it is compressed into another, in which there is very little or none, produces upon the two together no change of temperature, for there is no mechanical effect produced, because what would be gained in one is lost in the other. Separately considered, however, there is mechanical effect produced, though it cannot, as the experiment is made, be measured.

Let us, for example, conceive that to the vessel in which the air is compressed a long cylindrical pipe is attached, in which a piston is driven forward by the expanding air, the force with which the piston is driven forward constitutes the labouring force gained by the one vessel and lost by the other.

From what has been stated, it is not the actual amount of compression or expansion that determines the heat generated or lost, but the labouring force elicited in the compression or expansion. If it was not so, the same quantity of heat would be lost in the resumption of the original volume as was generated in the compression into a less. Now that by Mr. Joule's experiments is proved not to be so. Thus he compressed 2956 cubic inches of air into a 136½ cubic inches, and generated heat sufficient to raise a lb. of water to the temperature of above 13½°; but on expanding a 136½ cubic inches into 2859 by a peculiar process, he lost only heat enough to sink the temperature of a lb. of water 4°, or less than a third of what it ought to be if the steps of the former experiment had been retraced. Nevertheless, the same labouring force for a given amount of heat gained or lost was produced.

It follows from what we have said that if an air at a given constant temperature could be let out from one vessel into another so that the second vessel should enlarge just as fast as the gas came in, there would be no increase of heat in the second vessel, but there would be continual loss in the first from which the gas came. It is in this way Mr. Joule has made his latter experiments, and has produced results throwing completely out the effects that might be expected from the expansion of the air, and confirming the connexion with the labouring force.

We will now compare the theory with the experiments made by Mr. Joule. Before we do this, however, it will be necessary to reduce (13) to a more practical form. If our theory is correct, (13) ought to give not merely the law but the quantity of the labouring force. Now $t = m_{,,}s$, $m_{,,}$ being the mass of a particle of the N_1 particles, and $N_1\, m_{,,} = W$ the weight of the mass whose temperature is raised by the compression. Again, by (3) of the preceding Section $s = 1{\cdot}0896\,t$, or say $1{\cdot}09t$. Therefore the theorem (13) becomes

$$L = \frac{1{\cdot}09\,Wt}{2\sqrt{2}\,.\,m}(2T + t) \quad . \quad . \quad (14).$$

The only unknown quantity in this theorem is m, the mass of the particles of the compressing side. The greater these masses are, the less will be the labouring force for a given rise of temperature. If we suppose them to be equal to the mass of a particle of air $m = \frac{1}{1{\cdot}09}$, and the theorem becomes

$$L = \frac{1{\cdot}09^2\,Wt}{2\sqrt{2}}(2T + t) = {\cdot}4200375\,Wt(2T + t) \,..\, (15).$$

Instead of the full factor it will be sufficient to use ·42 only ; for going to greater accuracy in questions of this sort is mere affectation.

Experiment 1. By compressing 2956 cubic inches of air into $\frac{1}{21{\cdot}654}$th of the space occupied, Mr. Joule found, by a mean of six experiments, that the heat communicated to a mass of water, brass, and iron, equal in numeratom to 47·7929 lbs. of water, caused a rise of temperature, after due deductions, of 0°·285 Fahr., the barometer being 30·2 inches, and the thermometer 56°·2.

As the whole air, amounting to ·1309 lb., participated in the heat, I add that multiplied by 1·45 (the ratio of its numeratom to that of water) and the sum is 47·9827 lbs. of water, over which the heat of compression is diffused. But the temperature being 56°·2, $T = 1025$ very nearly, and

$$L = \cdot 42 \times 47\cdot 9827 \times \cdot 285 \times 2050\cdot 285 = 11776.$$

Mr. Joule, upon the supposition that the air follows the law of Mariotte, and that it is throughout the compression not sensibly heated, computes the labouring force at 11220·2 lbs. raised one foot high; that is, 559 lbs. less than our theorem from the data in the experiment gives. That Mr. Joule's computation should be beneath the mark is obvious, from the fact that he supposes the air not to be at all heated during the experiment. It is, indeed, but triflingly heated, yet, as the elasticity of it, and therefore the labouring force, increases as the square of the temperature rises, it must have some effect. Our result is just five per cent. above his.

Experiment 2. The same gentleman made a series of five similar experiments when the barometer was at 30·38 inches, and the thermometer at 57°·5, giving $T = 1026\cdot 2$, and compressed the air 10·498 times.

By following his mode of correction in the preceding experiment, I find the rise of temperature was 0°·1052 Fahr. I take it for granted he used the same quantity of water, &c. in both experiments; and as the effect of the air is so small on such a mass, though not more than half the quantity is used in the second experiment, I have for the sake of saving a few figures considered it the same, though the result will be somewhat larger than it ought to be.

By pursuing the same method of calculation, $L = 4351{\cdot}5$. Mr. Joule's result determined in the way in which it was in the former experiment is 4183·5, or 168 lbs. less than ours. The per centage of ours above Mr. Joule's, is 4.

In computing both these experiments, I have used the Fahr. differences of temperature as the true differences, very nearly which they are at the temperature of the experiments. But they are not accurately so, and give the numbers too small. On that account, and because a theorem in Fahr. temperature is easier, I here give it. Since

$$T^2 = \frac{448 + F}{480} 1{,}000{,}000$$

$$(T + t)^2 - T^2 = 2083\tfrac{1}{3}f \quad . \quad . \quad (16),$$

f being the rise of temperature in Fahr. Hence (13) becomes

$$L = \frac{\mathrm{N}_1}{2\sqrt{2m}} \times 2083\tfrac{1}{3}f = \frac{W \times 2083\tfrac{1}{3}f}{2\sqrt{2.mm_{\prime\prime}}} = 885\,Wf \;.\;.\;(17)$$

by supposing $m = m_{\prime\prime} = 1{\cdot}09$, as before. From this we obtain the following

LAW. *In the compression or rarefaction of any given portion of dry atmosphere, the labouring force of* *compression or that given out by expansion, is always proportional to the Fahr. rise or fall of temperature, which the gain or loss of heat diffused over a given quantity of water would occasion. Or the labouring force is equal to* 885 *lbs. raised one foot for every degree of Fahr. with which the increase or diminution of heat would raise or sink the temperature of a pound of water.*

The experimental discovery of this law is due to Mr. Joule. He has, however, imagined that the factor for the labouring force is 838 instead of 885. The reason, as before stated, of his result being too low is that he has not taken into account the rise, small as it is, of temperature in the compressed gas.

A slight error in his determination of the very small rise of temperatures, or in his correction of them, which amounts to more than half the rises observed, would be fully competent to annihilate all the difference. It indeed almost transcends our comprehension how that gentleman could have determined such very minute quantities amidst such a multitude of disturbing causes, so as to represent their effects with any degree of accuracy in the highly multiplied state in which we have them in the labouring forces.

The two preceding experiments computed by this law, the water and air being reckoned 47·8 lbs., come out 12056·3 and 4450·4. Mr. Joule's estimation being 11220·2 and 4183·5, or 6·4 and 6 per cent. too little.

The same philosopher made three other experiments—the first the mean of six experiments and each of the others the mean of seven—in a different way. He had before compressed the air, but he now tried the effect on expansion. His receiver of 136½ cubic inches had air compressed into it of many atmospheres tension. To the nosle of a tube communicating with it he screwed tightly on it "a coiled leaden pipe, a quarter of an inch internal diameter, and 12 yards long. The whole was then immersed into an oval can," filled with water, "covered at the top as tightly as possible. Having ascertained the temperature of the water by means of a very sensible thermometer, the stopcock was opened, and the air made to pass from the receiver, through" the long coil of pipe, most of it immersed in the can of water, and through "a pneumatic trough into a jar, by which it was carefully measured. After the air in the receiver had been reduced to the atmospheric pressure the water was again well stirred and its temperature noted. An interpolation was made after each of these experiments, in order to eliminate the effects of stirring," &c.

In this method of conducting the experiments it is presumed that all the loss of temperature, due to the expansion, was communicated to the water on account of the greater portion of the coiled leaden pipe being immersed in the can of water. And from the water in the jar into which the air passed, yielding to the air as it flowed in and keeping the pressure in it nearly constant, no gain or loss of heat accrued, as we have before shown. The entire loss therefore, or nearly so, arising from the expansion, was observed by the can with its contents; and the labouring force was simply that of raising a uniform weight, that of the pressure of the atmosphere, to a height equal to the length of the column which the air flowing out would occupy under the atmospheric pressure.

In the first of these experiments it is stated that "the cold was diffused through 21·17 lbs. of water, 14 lbs. of copper, 8 lbs of lead, and 7 lbs. of tinned iron," altogether equal to 23½ lbs. of water. I presume it is the same in each of the other experiments, since nothing else is mentioned.

Experiment 3. In the first of this new series of experiments 136 cubic inches of compressed air expanded, under a barometric pressure of 30·13 inches, to 2859, occasioning a loss of temperature when corrected of °·1738 Fahr.

Hence the labouring force should be, independent of temperature,

$$(2859-136)\times 30{\cdot}13\times{\cdot}49078\div 12=3355{\cdot}6\text{lbs.}$$

raised one foot high.

By our theorem it is

$$L=885\times 23{\cdot}5\times{\cdot}1738=3614{\cdot}6\text{ lbs.},$$

or 259 above the estimate. Consequently, the estimate is 7·2 per cent. below ours.

The air in this experiment was compressed nearly as much as it was in the first experiment, and it was but little less in quantity. Hence, if the expansion alone had controlled the labouring force, it should have been at least three times as much as it turns out to be.

Experiment 4. In a second experiment of the same kind, the barometer being 30·22 inches, 1384 cubic inches, compressed into 136, were allowed to escape as before, and the loss of temperature was 0°·081 Fahr.

Here the estimated labouring force is

$$(1384 - 136) \times 30{\cdot}22 \times {\cdot}49078 \div 12 = 1542{\cdot}5 \text{ lbs.},$$

raised one foot high.

Our theorem gives

$$L = 885 \times 23{\cdot}5 \times {\cdot}081 = 1684{\cdot}6 \text{ lbs.},$$

or 142·1 lbs. above the estimate. Hence the latter is 8·4 per cent. below theory.

Experiment 5. In a third experiment he had 3137 inches of air compressed into 136, but allowed only 1238 of it to be let out under a barometer of 30·2, and found the fall of temperature to be only 0°·0855.

Hence the estimated labour is

$$1238 \times 30{\cdot}2 \times {\cdot}49078 \div 12 = 1529{\cdot}2 \text{ lbs.},$$

and by the theorem

$$L = 885 \times 23{\cdot}5 \times {\cdot}0855 = 1778{\cdot}2 \text{ lbs.}$$

The difference is here no less than 249 upon very nearly the same quantity of air escaped as in the previous experiment, or nearly double the difference in the said experiment, and the per centage of the estimate 14·0 below theory. It appears to me that this points out very clearly the cause of the estimates being under the theory. We are now, it must be remembered, estimating loss of heat. In the 4th experiment 1248 inches of air are let out of about ten atmospheres compressed into one, and in the 5th, 1238, nearly the same quantity, is let out of about 23 atmospheres compressed into one. Now the two quantities of air being nearly the same, but the one coming from a much more compressed gas than the other, will expand more and occasion a greater loss of heat than the other, and consequently the loss of labouring force, estimated from neglecting to take into account that greater loss, must fall more short of the truth than the other, if the defect arises from a neglect of the heat lost. That is exactly the case. For the loss of temperature, in the liberation of the 1238 inches from the 23 atmospheres, is 0°·0855, and the defect from theory 249 lbs., while the loss of temperature in the liberation of 1248 inches from 10 atmospheres is only ·081, and the defect from theory 142 lbs. We hardly think a clearer proof could be had of the cause of Mr. Joule's estimate falling short of the theory. *Mutatis mutandis,* this is also the case why in compressions, as before observed, that the estimates are beneath the theory.

Mr. Joule has very properly remarked, that these experiments are destructive of the materiality of heat, and demonstrative of the theory of corpuscular motion. It is, I believe, impossible to reconcile the results of the third and fourth experiments with those of the first and second, upon any theory of the materiality of heat. In the third, for example, there was an expansion to 21 times the bulk, and heat enough liberated to raise a lb. of water 4°, while in the first a compression of only half a volume more, that is, 21½ volumes to 1, generated heat sufficient to raise the temperature of a lb. of water to above 13°½, or more than three times the amount of the former. Being converse effects, they ought to have produced the same results, very nearly, if heat was a substance, or "latent heat" had any effect.

It may perhaps be permitted me to say, that the striking agreement of all the quantities compared with the results of experiment, are satisfactory corroborations of the truth of our principles, and of the method followed in the inquiry. Candour will, I think, allow it to be next to impossible that any theory, borrowing nothing from experiment, could give laws and quantities so accordant with phenomena, under such different circumstances, if it was not the true one.

Besides by Mr. Joule, experiments have been made by several others. In these experiments it has been endeavoured to ensure accuracy by the very brief time of the duration of the experiment. But, according to our theory, such is the velocity of the particles of the atmosphere, that no time, however short, would be sufficient to eliminate interferences inseparable from experiments of this kind. The only way that has yet been tried to ensure success, appears to be that pursued by our Manchester philosopher. He has eluded the difficulty of time, by rendering the interferences themselves accountable for what they do; in short, as they so much exceed everything else, by making them the measure of the results.

MM. Clément and Desormes, some years back, occupied themselves with experiments to determine the effect of compression. They extracted air from balloons, until there was a trifling difference between the external h and internal h_1 pressures. Then, after the derangement in the temperature, which the extraction had occasioned, was restored, a hole in the balloon was suddenly opened, and the external air allowed to rush in, until the internal and external pressures again became equal, at which time the hole was closed. After allowing the excess of temperature caused by the rush in to dissipate, the new pressure h_{11} of the interior air was again taken. The quotient

$$\frac{h - h_{11}}{h_{11} - h_1}$$

was then found to be about one third, but varying irregularly from it in different experiments.

The duration of this experiment, it seems, was little more than half a second. Short as this is, it is sufficient to upset all conclusions upon the supposition that the air alone is affected by the heat generated. For the ordinary velocity of the air being about 1110 feet per second, would transmit any difference of temperature above 500 feet in that time. Therefore, a particle of air in half a second would rebound from side to side of a balloon two feet diameter 250 times, and of course would communicate and receive any difference of temperature with the sides of the balloon as many times. No dependence, therefore, on experiments made in this way, can be placed, unless one can tell how much matter of the containing vessel participated in the heat generated.

From (8) we may compute the rise in temperature of any compression, on the hypothesis of the air alone receiving the heat, by putting $W = 0$ in the value of q, which will reduce it to

$$q = \frac{m}{\sqrt{2} \, . \, g m_1} = \frac{1}{45{\cdot}537} = {\cdot}02196$$

when $m = m_1$. If, therefore, the gas be compressed twice, that is $\frac{V}{v} = 2$, we find $t = 15{\cdot}7$, when $T = 1029$, or about 60° Fahr. Then, since t nearly corresponds to Fahr. degrees at such a temperature, the rise due to compression of a gas into half the space, is $15^\circ{\cdot}7$ Fahr., not 50°, as estimated by the comparative process of Dalton. It is, indeed, as we have over and over remarked, impossible by any such a process as he has adopted to arrive at the truth, or near it.

Whether there be any more general solution of the problem than is given above in (7) and (8), it is difficult to say. We have no satisfactory experiments, that I know of, to lead us to believe that any more general is required; and without something conclusive to guide us it would be a mere waste of time to look further. It may, however, not be superfluous to observe, that should any experiments be instituted to show a greater increase of temperature than our theorem gives when air alone is compressed, it will merely require an alteration in the coefficient of *A* in the denominator of (7), to accord with it. Thus, if the said coefficient was one third of its present, it could scarcely affect the results of the other experiments, and yet give 47° rise for the compression of air only to one half its bulk. Why that coefficient should be so altered no reason has yet presented itself to me. I merely notice it to show the flexibility of the theorem.

The evolution of positive and negative heat by compression and rarefaction is yet in a very imperfect state, and wants new and varied experiments. It has been stated to me that the escape of steam of several atmospheres tension, through a cock or aperture into the atmosphere, will create a diminution of temperature sufficient to freeze water; but I have never heard of any experiments to confirm it. Once, indeed, I put my hand in some steam issuing from a vessel, said to be at two or three atmospheres pressure, and there was a very sensible cold, but nothing approaching the freezing of water.

It was also stated by one of the atmospheric party, at a meeting of the Institution of Civil Engineers, that the air pumped out of the tube, was when issuing from the air-pump, at a temperature above boiling water. But all that party dealt very much in the marvellous.

In scanning very recently the Proceedings of the sections of the British Association for 1845, p. 31, I perceive Mr. Joule, by some new experiments, has found a number similar to our 885 in (17), as I understand it, equal to 890. That is, that the labouring force necessary to raise 1 lb. of water 1° Fahr. is equal to the *vis viva* required by a weight of 890 lbs. falling one foot of altitude.

It would appear by these experiments that the labouring, or mechanical force, necessary to raise the temperature of 1 lb. of water 1° Fahr., is equivalent to that of lifting 885 lbs. 1 foot high. Though this be true in these experiments of mechanical power, it is by no means so in the application of fuel. Thus, if we take the fuel necessary to raise a cubic foot of water 32° Fahr. to 212° Fahr. as 1, the quantity that would convert that cubic foot of water into steam at 212° Fahr. is, by Scholium 2, Proposition V, Section 5, Book II, 5·658, and the labouring force generated by that steam 3,600,000 raised one foot. But if c be the quantity of fuel necessary to raise a lb. of water one degree of true temperature, which, between water freezing and boiling, is nearly equivalent to a degree of Fahr., we have $1 = 172 \cdot 6 \times 62 \cdot 5 \times c$, 172·6 being the number of degrees of true temperature between freezing and boiling, and 62·5 lbs. the weight of a cubic foot of water. Consequently, $5{\cdot}658 \times 172{\cdot}6 \times 62{\cdot}5 \times c$, gives a labouring force of 3,600,000 lbs., and

$$c \text{ gives } \frac{3{,}600{,}000}{5{\cdot}658 \times 172{\cdot}6 \times 62{\cdot}5} = 59 \text{ about.}$$

That is, the fuel necessary to raise a lb. of water $\frac{180}{172{\cdot}6}$, or above one degree of Fahr., would produce a labouring force of only 59 lbs., or 15 times less than the mechanical force required to produce the same thermometric effect, on the same quantity of water. Fuel, therefore, would be much more economical in raising the temperature than mechanical or labouring force.

SECT. VI.

LAWS OF TEMPERATURE IN ATMOSPHERES OR SUPERINCUMBENT MEDIA.

General Observations.

Because the particles of an atmosphere are constantly solicited by the body which they envelop, their velocities from the body, or as we should say upwards, are invariably diminished, and downwards increased. Hence it follows, that they strike all particles beneath them with an increased, and all above them with a diminished force; so that the higher we ascend in the atmosphere the less is the velocity of the particles, and the greater the lower we descend. But, according to our definition of heat, the temperature is proportional to the individual momentum of the particles; that is, in particles of equal mass, to their velocity. Consequently, the temperature of such atmospheres depends on the temperatures at the surfaces of the central bodies, and diminishes as the distances from these surfaces increase.

Thus the cause of the decrease of temperature in the atmosphere, is not attributable to any superior capacity of the higher strata of the air for heat, nor solely to their distance from the earth, but to that very force which retains the atmosphere about the body; namely, its gravitation.

The nature of the decrease of temperature being understood that of the decrease of density and pressure is exceedingly simple. For as all parts of the air are attracted towards the body it surrounds, each stratum has to resist the force with which all those that are above it are attracted. Therefore, the lower the stratum the more it is compressed, and the greater will be its density and elasticity.

PROPOSITION I.

If any gas surrounding a solid body has its particles solicited towards the latter, by a force which is any function of the altitude, and its particles derive their motion entirely from the surrounded body, it is required to determine the velocity of the particles at any altitude.

It is immaterial in whatever way we conceive the particles to move, whether obliquely or directly towards or from the solid body; for it is well known the effect of this body on the velocity of a particle is the same for an equal approach, in an oblique, as in a direct course. We shall therefore, and for the sake of simplicity, assume that the motions of the particles are made in lines directly to and from the soliciting body, or centre of attraction. Conceive then that there are two strata of particles keeping each other in equilibrio, by motions in right lines passing through the attracting centre, and let V be the mean velocity of a particle in the lower stratum, and $V_{,}$ that of a particle in the upper. Then if N, $N_{,}$ be the corresponding numeratoms

$$V : 1^s :: \frac{2}{N^{\frac{1}{3}}} : \frac{2}{VN^{\frac{1}{3}}}{}^{s}, \text{ and } V_{,} : 1^s :: \frac{2}{N_{,}^{\frac{1}{3}}} : \frac{2}{V_{,}N_{,}^{\frac{1}{3}}}{}^{s}$$

the times of a whole revolution of a particle of each. Consequently $\frac{VN^{\frac{1}{3}}}{2}$ and $\frac{V_{,}N_{,}^{\frac{1}{3}}}{2}$ are the numbers of revolutions in 1^s. And if $\dot{V}$ and $\dot{V}_{,}$ be the differences between the extreme and mean velocities of the two particles respectively, the equilibrious action of one particle upwards will be $\frac{VN^{\frac{1}{3}}\,(V-\dot{V})}{2}$, and of the other downwards $\frac{V_{,}N_{,}^{\frac{1}{3}}\,(V+\dot{V}_{,})}{2}$, which two quantities must be equal. Therefore,

$$V_{,} = \frac{VN^{\frac{1}{3}}}{V_{,}N_{,}^{\frac{1}{3}}}(V - \dot{V}) - \dot{V}_{,};$$

and putting $V_{,} = V - v$,

$$-v = V_{,} - V = \frac{VN^{\frac{1}{3}} - V_{,}N_{,}^{\frac{1}{3}}}{V_{,}N_{,}^{\frac{1}{3}}}V - \frac{VN^{\frac{1}{3}}}{V_{,}N_{,}^{\frac{1}{3}}}\dot{V} - \dot{V}_{,}.$$

And since $\dot{V}$ is generated in $\dfrac{1}{2VN^{\frac{1}{3}}}$ time or a quarter of a revolution, and $\dot{V}_{,}$ in $\dfrac{1}{2V_{,}N_{,}^{\frac{1}{3}}}$ time, the last two numbers of the preceding equation are equal to $-2\dot{V}_{1}$, that is very nearly $-2\dot{V}$. But if we substitute in the numerator of the first member, on the right hand of this equation, for $V_{,}$ its value $V - v$, and $N - n$ for $N_{,}$, this numerator becomes

$$V\left\{\frac{nV}{3N^{\frac{1}{3}}} + N^{\frac{1}{3}}v\right\},$$

rejecting the higher powers of n and the products of n and v, as inconsiderable. Consequently,

$$-v = \frac{nV}{3N} + v - 2\dot{V} = \frac{nV}{6N} - \dot{V} \;\ldots\; (1).$$

Again, because of the attraction, the mean velocity of a particle in N exceeds that with which it left the higher stratum $N_{,}$ by $\dot{V}$. So that on this account N is less than it would be if the particles of it retained the velocity only with which they left those of $N_{,}$. On the other hand, the constant solicitation of the particles towards the central force, tends to compress the stratum, and thereby augment the numeratom N. Let us suppose, therefore, that AN is the numeratom which this stratum would have, to enable it to maintain the same action upwards, were it freed from the influence of the attracting body, and its particles had a uniform velocity equal to $V - \dot{V}$, that with which they leave $N_{,}$. In the same way call $A_{,}N_{,}$ the numeratom of the contiguous superior stratum, in like manner freed from the attraction of the solid body, and reduced to maintain its proper action upwards with a uniform velocity to its particles equal to $V_{,} - \dot{V}_{,,}$, that with which they leave the next superior stratum $N_{,,}$.

Now the motion of the particles of AN in a given time is $AN . \frac{(V - \dot{V})^2}{2}$, and that of $A_{,}N_{,}$ is $A_{,}N_{,} . \frac{(V_{,} - V_{,,})^2}{2}$. which are obviously the respective actions of the strata N, $N_{,}$ upwards, previous to depriving them of the attraction of the central body. If, therefore, the former action be reduced, in the ratio of $(V_{,} + \dot{V}_{,,})^2$ to $(V_{,} - \dot{V}_{,,})^2$, or which is very nearly the same, in the ratio of V^2 to $(V - 2\dot{V})^2$, it will be competent to support the stratum $A_{,}N_{,}$ with the velocity with which its particles leave $N_{,,}$, of course giving

$$AN \frac{(V - \dot{V})^2}{2} \times \frac{(V - 2\dot{V})^2}{V^2} = A_{,}N_{,} \frac{(V_{,} - \dot{V}_{,,})^2}{2}.$$

But because A and $A_{,}$ are respectively the same functions of V, $\dot V$ and $V_{,}$, $\dot V_{,}$, they are very nearly equal; and for the same reason so are the other corresponding factors of N, $N_{,}$. We have, hence,

$$N_{,}=N\frac{(V-2\dot V)^2}{V^2}=N\frac{V-4\dot V}{V},$$

and $$4\dot V N=(N-N_{,})V=nV.$$

Substituting this in (1) gives $v=\frac{\dot V}{3}$. But $\dot V$ being the velocity generated by the attraction in $\frac{1}{2VN^{\frac{1}{3}}}$ time, if g denote the velocity that would be generated in a particle of N by the force acting on it at the end of 1^s, $\dot V=\frac{g}{2VN^{\frac{1}{3}}}$, and

$$v=\frac{g}{6VN^{\frac{1}{3}}} \quad . \quad . \quad . \quad (2)$$

Moreover, since V is constantly varying, owing to the attraction of the solid body on the particle, and likewise $V_{,}$, and v is the mean of all the simultaneous differences between V and $V_{,}$, the entire period which v takes to run through all its values, is, at a medium, half the sum of two mean revolutions of two consecutive particles, one in the stratum N the other in $N_{,}$; or, which is very nearly the same, it is the time $\frac{2}{VN^{\frac{1}{3}}}$ of a revolution of a particle in N.

If, therefore, θ denote the time elapsed from the communication of motion from the central body to its arrival at the stratum N, at x distance from the centre of attraction, $v : \frac{2}{VN^{\frac{1}{3}}} : : dV : -d\theta = \frac{2dV}{vN^{\frac{1}{3}}V}$.

But V being the velocity $-d\theta = -\frac{dx}{V}$; whence $\frac{2dV}{vN^{\frac{1}{3}}} = -dx$, and combining it with (2),

$$2VdV = -\frac{gdx}{6} \quad . \quad . \quad . \quad (3)$$

a general equation for the velocity in terms of the attracting force g and altitude x. If for g we put fx, the function which the accelerative force is of x, and represent the values of V and x at the origin of the integral by $\mathbf{V}$ and X, we have

$$6(\mathbf{V}^2 - V^2) = \int fx \, . \, dx - \int fX \, . \, dX \quad . \quad . \quad (4).$$

Cor. 1. If the attraction be the same at all altitudes $fx = g$ some constant; and if x be the distance from the surface of the body $fX = 0$, and (4) becomes simply

$$6(\mathbf{V}^2 - V^2) = gx \quad . \quad . \quad (5).$$

Cor. 2. At the surface or highest point of the gas $V = 0$, leaving

$$6\mathbf{V}^2 = gx.$$

So that if we knew the values of $\mathbf{V}$ and g we could at once determine the total altitude of the air, which would be $= \frac{6\mathbf{V}^2}{g}$.

Cor. 3. Because the density or weight of air is not taken into account, the altitude of the atmosphere is independent of the quantity of air, and would be the same were there much or little air.

Cor. 4. But since the altitude is inversely as g, the accelerative velocity of a particle due to the attraction of the solid, it would be greater the less the attraction, and less the greater the attraction, that is, simply in the inverse ratio of the intensity of attraction.

Cor. 5. And because the altitude is as the square of the velocity at the surface, and this square, *Cor.* 3, Proposition VIII, Section 2, Book II, in homogeneous gases at the same temperature and pressure, is as the specific gravity of the gas inversely, the total altitudes of two atmospheres, would, under the same circumstances, be inversely as the specific gravities of the component airs.

Thus, an atmosphere of hydrogen would be about 16 times higher than one of oxygen, and about $14\frac{2}{5}$ times higher than one of a gas such as our atmosphere is composed of.

Cor. 6. Since the altitude is as V^2, which is proportional to the Fahr. temperature plus 448°, the altitude is higher, the higher the temperature at the surface; and the difference of altitude is directly as the difference of this surface temperature in Fahr.

Cor. 7. If the solid body be a globe whose radius is r, and its accelerative gravity at the surface be g, and at any other distance x from the surface directly as the distance from the centre, $fx = g\,\frac{r+x}{r}$, and

(4) becomes

$$6(\mathrm{V}^2 - V^2) = g\,\frac{2rx + x^2}{2r} \quad . \quad . \quad . \quad (6),$$

from which $x = \sqrt{\frac{12r}{g}(\mathrm{V}^2 - V^2) + r^2} - r.$

Cor. 8. The same things supposed, but that the attraction varies as the square of the distance inversely from the centre of the globe $fx = g\,\frac{r^2}{(r+x)^2}$, and therefore

$$6(\mathrm{V}^2 - V^2) = \frac{grx}{r+x} \quad . \quad . \quad . \quad . \quad . \quad . \quad (7).$$

Whence,

$$x = \frac{6r\,(\mathrm{V}^2 - V^2)}{gr - 6\mathrm{V}^2 + 6V^2},$$

and the total altitude of the atmosphere is $\frac{6r\mathrm{V}^2}{gr - 6\mathrm{V}^2}$.

If, therefore, $6\mathrm{V}^2 = gr$, the altitude is infinite, or rather the atmosphere would be wholly dispersed.

Now, this may happen two ways, either by the increase of V or the diminution of r or g. In the former case V may become greater by an increase of temperature to which it is proportional, or by changing the gas to one of less specific gravity. In the latter case, the dimensions of the globe, or its intensity of attraction, or both together, maybe diminished, so as to reduce the denominator to 0 or a negative quantity.

Hence there are four circumstances which prescribe limits to the possibility of retaining an atmosphere about a globe, whose law of attraction follows the inverse duplicate ratio of the distances, namely—the intensity of its attraction—the dimensions of the globe—the specific gravity of the gas—and the temperature of the globe. If the three first are severally or conjointly too small, or the last too large, no atmosphere can exist.

Cor. 9. Let us suppose $fx = g\,(r + x)^n \div r^n$, and

$$6(\mathbf{V}^2 - V^2) = g\,\frac{(r + x)^{n+1} - r^{n+1}}{(n + 1)\,r^n} \quad . \; . \; . \; (8),$$

which, when $n = -1$, becomes a vanishing fraction whose value is

$$6\,(\mathbf{V}^2 - V^2) = gr \log \frac{r + x}{r}.$$

PROPOSITION II.

The attraction being the same at all altitudes, and the specific gravity being given, to determine the rate of the decrease of temperature, and the altitude of the air.

Setting aside the influence of the solid body during the period of a particle, let V be the velocity as before, and M the mass of a particle, n the numeratom, e the elasticity, and t, f, c, the true, Fahr., and Centigrade temperatures of the air at the altitude x; and $\mathbf{V}$, M,

N, T, F, C, the like quantities at the surface, or 0 altitude. Then MV being the momentum of a particle, $2MV$ is the force of collision with another particle meeting it with an equal opposite momentum MV, and may therefore be considered as the motion communicated by the particle at each collision. Consequently, $2MV \times \frac{Vn^{1/3}}{2}$, the number of collisions in a given time, 1^s for instance, is the motion communicated by a particle in 1^s. And if this be drawn into $n^{\frac{2}{3}}$, it will give MV^2n, the motion communicated by the whole stratum in 1^s. But e being the absolute mass that would be supported by the gas, if g as before be the accelerative velocity at the altitude x, eg is the motion which this action of the gas would communicate at the end of 1^s. Therefore,

$$ge = MV^2n = V^2w,$$

w being the mass of unity volume of the gas. If e be measured by the height b of the barometer, w will be the mass of a column of air of equal dimensions at a temperature corresponding to V. Taking, as we have supposed, f for the Fahr. temperature, and m for the ratio of the mass of a given volume of mercury to an equal volume of air at the same temperature of melting ice and barometric pressure b,

$$e = bmw\frac{f+448}{480};$$

for mw is the mass of unity column of mercury, and bmw of the whole column.

Whence,

$$V^2 = gbm\frac{f+448}{480} \quad \ldots\ldots \quad (9)$$

and by (5)

$$x = bm\,\frac{F-f}{80} \quad \ldots\ldots \quad (10).$$

Now, according to M. Biot (Traité de Physique, tom. ii, p. 15), if $b = \cdot 76$ metres $= \cdot 76 \times 3\cdot 28085 = 2\cdot 493446$ English feet, $m = 10463$ (Traité, tom. i, p. 408) at Paris. Therefore,

$$\left.\begin{aligned} x &= 326\cdot 1115687\,(F-f) \text{ English feet} \\ &= 108\cdot 7038562\,(F-f) \quad \text{Do.} \quad \text{yards} \end{aligned}\right\} \;.\;.\; (11).$$

That is, the temperature in Fahr. decreases uniformly at the rate of 1° for every 108·7038562 yards. And the total altitude of the air, supposing the true temperature F to be 52°, is $108\cdot 7038562 \times (52+448) = 543519\cdot 2810$ yards $= 30\cdot 881777$ miles.

These results are on the supposition of a homogeneous and perfectly dry air.

Cor. 1. It would appear at first from (10) that the force of gravity g not appearing, in this reduced expression for the temperature and altitude, has no influence in the rate of decrease of temperature. But if we reflect, that m would be the same, however much the value of g change, provided the quantity of gas remain the same, and that b itself must increase or diminish, in order to preserve the same pressure, in the inverse proportion of the value of g, we shall at once perceive the influence of g though not virtually expressed. Indeed the amount of diminution in Fahr. temperature will be inversely as b, or directly as the intensity of attraction, the altitude being the same.

Cor. 2. And the rate of decrease of temperature, other things being alike, is not at all affected by the temperature at the surface, be it high or low.

Cor. 3. There is, however, another cause which produces considerable effects on the rate of diminution of temperature, namely, the specific gravity of the gas. In two gases of unequal specific gravities, the standard of comparison being mercury, m will be greater in the atmosphere with the lighter gas than in the atmosphere with the heavier, in the inverse ratio of the specific gravities of the gases. Therefore in the lighter gas the rate of diminution of temperature will be less, in proportion to the specific gravity of the gas.

PROPOSITION III.

Other things being alike, let it be required to compute the rate of diminution of temperature, in an atmosphere surrounding a globe of given dimensions, when the law of attraction is inversely proportional to the squares of the distances.

We have seen by (9) that $V^2 = gbm\frac{f+448}{480}$ in a uniform gravity. Therefore, $\mathbf{V}^2 = gbm\frac{F+448}{480}$, in our present Problem, at the surface; and

$$6\,(\mathbf{V}^2-V^2)=6gbm\left(\frac{F-f}{480}\right)=gbm\left(\frac{F-f}{80}\right)..(12).$$

For V being independent of the variation of the attractive force, has obviously the same factor as **V.** Hence, by (7)

$$bm\,\frac{F-f}{80}=\frac{rx}{r+x}$$

and

$$x=\frac{rbm\,(F-f)}{80\,r-bm\,(F-f)} \qquad (13).$$

Equations which give the rate of the diminution of temperature, and the altitude when the diminution is given; r being the radius of the globe, and b, m the same quantities, and, on our earth, having the same values as in the last Proposition.

Cor. 1. If A be the altitude computed on the hypothesis of a uniform gravity for the same fall of temperature $F-f$, we have

$$x=\frac{rA}{r-A}=A\left\{1+\frac{A}{r}+\frac{A^2}{r^2}+\frac{A^3}{r^3}+..\right\}..(14).$$

So that if $r=3958$ miles the radius of our earth, and $A=30{\cdot}88$ miles as in the last Proposition,

$$x=\frac{3958\times30{\cdot}88}{3958-30{\cdot}88}=31{\cdot}12\text{ miles},$$

or about $\frac{1}{4}$ mile more than the computation of a uniform gravity.

Cor. 2. By taking the reciprocal of r in yards, we shall find

$$x = A + \cdot 00000014355\,A^2 \quad . \;\; . \;\; . \quad (15)$$

rejecting the succeeding terms as insignificant. If therefore

A is

1000 2000 3000 4000 5000 6000 7000 8000 9000 10000

yards, the defects of altitude on the hypothesis of a uniform gravity will be in yards

·144 ·574 1·292 2·297 3·589 5·168 7·034 9·187 11·628 14·355

These quantities are very minute when one considers the altitudes to which they apply.

Cor. 3. Small as the error in the altitude is, on the assumption of a uniform gravity, the effect on the temperature is much less. It could not indeed be appreciated, scarcely in a long series of the best observations. In an altitude of 10,000 yards, where the depression would amount to 92°, it would not rise to 0°·13; and in an altitude of 7000 yards, the height nearly to which Lussac ascended, where the depression would be 64°, it would scarcely exceed 0°·06. At lower altitudes it would be still less in the duplicate ratio of the altitude.

Proposition IV.

The same things being supposed, it is required to compute the rate of the temperature's diminution when the force varies as any power n of the central distance.

Having found the value of $\mathrm{V}^2 - V^2$ in (12) of the preceding Proposition, we have at once from (8) the solution of the present Proposition, namely,

$$bm\,(F-f)=80\,\frac{(r+x)^{n+1}-r^{n+1}}{(n+1)\,rn} \quad . \quad . \quad . \, (16).$$

Cor. If we put $n=-2$ it will sink to (13), and by making r infinite (10) is readily deduced. But this has been so fully treated of in Proposition II, that it is unnecessary to say more than to observe, that whatever is there said of the squares of the velocities, may be immediately applied to the Fahr. temperatures, by merely substituting for V^2-V^2 its value in (12).

General Scholium.

COMPARISON OF THE PRECEDING THEORY, WITH OBSERVATIONS.

It has been shown in *Cors.* 2 and 3 to Proposition III, that treating the law of gravity as uniform, that is, neglecting the decrease of attraction, in any altitudes to which we can ascend, leads to no sensible errors either in altitude or temperature. But should it even be needful to allow for them, it is so easily done, that it would be useless to trouble ourselves in the computations with more than the simple formulæ resulting from the supposition of a uniform gravity.

On this principle the 11th and 12th cols. of Table 3 are constructed, showing the elevations for the several degrees of Fahr. and Centigrade depressions. And for the sake of those who require the nearest degree of accuracy, I have added the 13th col., giving the yards to be added to make up the true altitude when the decrease of force is taken into account. But it should be observed that when the Table was computed, it was thought accurate enough to reckon the altitude 108·7 yards for every degree depression of Fahr. the remaining figures being neglected. This table is therefore deficient, at the rate of 0·355 yards in every 10,000.

Our computations rest on the supposition of a clear altitude from the surface, and a freedom from the effects of radiation. Should there be any eminences as hills, particularly if of large extent, it is probable that they would exercise some influence on temperatures taken on them. For the heat of the surface results from the action of the solar rays and not from the surface's distance from the centre of the earth, which has nothing to do with it. The heat at the surface on which the gradations of heat in the atmosphere depend, would be just the same were that surface on a level and hundreds of miles higher or lower than it is. Any large extent of ground must, therefore, affect the temperatures that would be given for an elevation above the mean surface of the earth. But on account of the direct communication of heat through the mountains from the earth, and their greater bulk near their roots, it is obvious these effects would be proportionally more sensible in low than in lofty elevations. Observation proves this to be the case; but, considering the vast magnitude of some hills, it is surprising to see how comparatively trifling the deviations from theory are. The following table of depressions, in the seventh volume of Thomson's 'Annals,' p. 383, is from Humboldt, and, with the computed depressions annexed for the several heights, will illustrate the truth of our remarks.

S. America, between lats. 10° N. and 10° S.						Mexico, between lats. 17° and 21° N.			
Heights in French Fathoms.	Heights in English Yards.	Names of Places.	Mean Ann. Temp. Centigrade. Observed.	Mean Ann. Temp. Centigrade. Computed.	Mean Ann. Temp. Centigrade. Diffs. Temp.	Names of Places.	Mean Ann. Temp. Centigrade. Observed.	Mean Ann. Temp. Centigrade. Computed.	Mean Ann. Temp. Centigrade. Diffs. Temp.
			°	°	°		°	°	°
0	0	Cumana .	27·5	27·5	0·0	Vera Cruz . .	26·0	26·0	0·0
500	1066	Caracas .	20·5	22·1	+ 1·6	Encero	19·8	20·6	+ 0·8
1000	2132	Popayan .	18·0	16·6	— 1·4	Valladolid . .	18·0	15·1	— 2·9
1500	3197	Quito . .	13·5	11·2	— 2·3	Real del Monte	14·0	9·7	— 4·3
2000	4263	Unknown	6·8	5·7	— 1·1	Unknown . .	7·5	4·2	— 3·3
2500	5329	Pinchinca	1·5	0·3	— 1·2	Ditto . . .	— 1·0	— 1·2	+ 0·2

These examples abundantly confirm our deductions, the deviations being so inconsiderable as to be undeserving of notice in matters so liable to error and irregularity from local circumstances. Cities and large towns, as some of the above are, afford not the best opportunities for determining the true mean temperatures of the regions in which they are situated. The temperatures of such places must always appear higher than they would if there were no such habitations.

The above observations present an accordance with theory nearer than in such things we could well expect; but the following collection of observations, extracted from M. Ramond, on the "Baromerin Formula," p. 189, comes more to the point still:

Names of Higher Stations.	Names of Lower Stations.	Altitudes, Eng. yds.	Temperatures Centigrade. Lower Station	Temperatures Centigrade. Higher Station.	Differences. Observ.	Differences. Comp.	Errors. Parti-cular.	Errors. Mean.
			°	°	°	°	°	°
Gay Lussac's ascent	Paris	7630	30·8	— 9·5	40·3	39·0	— 1·3	— 1·3
Chimborazo . .	South Sea . . .	6427	25·3	— 1·6	26·9	32·8	+ 5·9	+ 5·9
Mont Blanc . .	Genève, noon . .	4782	28·3	— 2·9	31·2	24·4	— 6·8	—5·8
Ditto . . .	2 hrs. p.m.	...	27·6	— 1·6	29·2	...	— 4·8	
Pic de Ténériffe .	Orotava (by Cordier)	4077	24·9	+ 8·4	16·5	20·8	+ 4·3	+ 4·3
Mont Blanc . .	Chamouny, noon .	4070	23·0	— 2·9	25·9	20·8	— 5·1	—5·5
Ditto . . .	2 hrs. S.	...	25·0	— 1·6	26·6	...	— 5·8	
Etna	Catana (by Saussure)	3649	23·1	+ 4·4	18·7	18·7	0·0	0·0
Mont Perdu . .	Tarbes	3408	25·6	6·9	18·7	17·4	— 1·3	— 1·3
Col du Géant . .	Genève	3346	24·9	4·5	20·4	17·1	— 3·3	— 3·3
Maladette . . .	Tarbes (by Cordier)	3174	20·8	3·4	17·4	16·2	— 1·2	— 1·2
Pic du Midi . .	Tarbes, July 26, 1809	2858	27·5	11·6	15·9	14·6	— 1·3	+1·6
Ditto . . .	Sep. 15 . .	...	19·6	8·6	11·0	...	+ 3·6	
Ditto . . .	Sep. 4, 1803	...	22·5	8·1	14·4	...	+ 0·2	
Ditto . . .	Sep. 12 .	...	23·5	10·4	13·1	...	+ 1·5	
Ditto . . .	Sep. 23 .	...	18·8	8·1	10·7	...	+ 3·9	
Ditto . . .	Sep. 27 .	...	19·1	4·0	15·1	...	— 0·5	
Ditto . . .	Sep. 30 .	...	14·8	4·3	10·5	...	+ 4·1	
Col du Géant . .	Chamouny . . .	2606	21·6	4·5	17·1	13·3	— 3·8	— 3·8
Mont Perdu . .	Barèges . . .	2354	25·0	6·9	18·1	12·0	— 6·1	— 6·1
Pic d'Eyré . .	Tarbes	2347	21·3	11·0	10·3	12·0	+ 1·7	+ 1·7
Guanaxnoto . .	South Sea . . *	2279	25·3	21·3	4·0	11·6	+ 7·6	+ 7·6
Pic de Montaigu .	Tarbes	2244	14·5	3·1	11·4	11·5	+ 0·1	+ 0·1
Pic de Bergons .	Ditto . . . *	1959	19·0	13·5	5·5	10·0	+ 4·5	+ 4·5
Pic du Midi .	Barèges Aug. 30, 1805	1808	26·7	16·4	10·3	9·2	— 1·1	—3·3
Ditto . . .	Sep. 15 .	...	21·9	8·0	13·9	...	— 4·7	
Ditto . . .	Aug. 15, 1809	...	21·3	8·2	13·1	...	— 3·9	
Ditto . . .	Sep. 23 .	...	18·5	6·0	12·5	...	— 3·3	
Ditto . . .	Oct. 19 .	...	15·9	2·5	13·4	...	— 4·2	
Ditto . . .	Sep. 11, 1810	...	17·8	7·0	10·8	...	— 1·6	
Ditto . . .	Sep. 22 .	...	18·9	5·8	13·1	...	— 3·9	
Ditto . . .	Sep. 28 .	...	18·4	5·2	13·2	...	— 4·0	
Puy de Dôme .	Clermont June 25, 1806	1163	21·3	14·4	6·9	5·9	— 1.0	—2.1
Ditto . . .	Oct. 11, 1807, noon	...	17·8	10·8	7·0	...	— 1·1	
Ditto . .	1 hr. S.	...	18·6	11·7	6·9	...	— 1·0	
Ditto . . .	June 29, 1808	...	24·8	15·2	9·6	...	— 3·7	
Ditto . . .	Aug. 7 .	...	32·9	23·4	9·4	...	— 3·5	
Mounne du Bagnère	Tarbes . . . *	1039	10·3	9·6	0·7	5·3	+ 4·6	+ 4·6
Bedat du Bagnère	Ditto	611	10·9	8·0	2·9	3·1	+ 0·2	+ 0·2
Pont du Berger .	Clermont . . .	537	0·3	—2·9	3·2	2·7	— 0·5	— 0·5
La Barraque . .	Ditto	415	23·6	+21.8	1·8	2·1	+ 0·3	+ 0·3
Prudelle . . .	Ditto . . . *	314	28·3	25·0	3·3	1·6	— 1·7	— 1·7
							Mean error	—0·22

Thus the mean of all the mean differences is only about 2-9ths of a degree Cent., an agreement which much surpasses that of the different observations at the same place; for at Pic du Midi and Tarbes the differences amount to no less than 5°·4 in an observed depression of only 13° or to 5-12ths of the whole; and it is not much better at Pic du Midi and Barèges.

The four observations marked with an asterisk are rejected by M. Ramond, in consequence of their differing more than the rest in the rate of diminution of temperature which they give. But this method of exclusion ought to be managed with a very prudent hand. However, if we exclude the same four observations, our theory will fall 1°·06 short instead of ·22. Even this is several times within the limits which the observations themselves show that they can be depended on. The deviation, too, of the theory is a deficiency in rate just as one might expect. For the theory, being exclusive of radiation, cannot perfectly represent phenomena when this exists.

Radiation is more powerful the nearer the surface, and its influence proportionally greater; so that if the earth be warmer than the air above it, which it usually is, both thermometers will show a higher temperature than belongs to the air, but that nearer the earth will show the greater excess. Thus the diminution of temperature will appear to march more rapidly than it really does; and more so in low altitudes than in high. Now this is so far the case that many suppose the diminution of Fahr. temperature is not equable but less the higher we ascend.

Mr. Ivory, who has examined this subject, says (Philosophical Magazine for October 1825, p. 247), that if "the experiments recorded by Ramond be partitioned in two classes—one containing 16, in which the height is less than 2000 yards, and the other 22 of greater altitude—the mean height for depressing the Centigrade thermometer one degree deduced from the first class is 144 metres, or 78 fathoms; and the same height in the other class is 180 metres, or 98 fathoms. The difference of these two determinations is considerable, and proves incontestably a retardation of the decrease of heat in ascending."

That Mr. Ivory's inference is just, no one can doubt, and being drawn from phenomena which are the compound effects of radiation, the natural diminution due to the atmosphere, and local affections, confirms what we have already said. However, all conclusions from experiments at low altitudes, in matters exposed to so many disturbances and subject to such uncertainty, should be made with considerable allowances. We should remember that local circumstances would have nearly the same influence in low altitudes as in high, while the amount of depression being much less, their proportionate disturbance would be much greater. This is manifest from the preceding collection of observations. For the mean differences from our theory are considerably less in the minor elevations than in the superior—in some of the most inferior they are particularly small—yet, as Mr. Ivory has found, the low altitudes exhibit a diminution of only 78 fathoms for 1° Cent., that is, full 20 fathoms less than is given by the higher altitudes.

Another circumstance which contributes powerfully to the irregularity of the observations, is the time at which they are usually made; namely, in the day and about noon. At this time radiation is the greatest, and its effects consequently are most sensible. In the night, or a little after sunset, Dr. Wells found that instead of a decrease of temperature there was, at least for several feet high, a most rapid increase, to the extent, for instance, of 10 to 12, 14, or 16 degrees of Fahr. This, probably, does not reach to any great height; but it would be enough to affect materially the rate of decrease in the highest altitudes, and seems to show very plainly that the fall of temperature in the night and in the day is not the same. Our theory, which regards the air in its mean state, would probably bisect the difference between the two.

It will be seen, by the preceding quotation from Mr. Ivory, that 22 experiments, at heights exceeding 2000 yards, give at a medium one degree Cent. for 98 fathoms. Now by our tables 1° Cent. corresponds with 97·83 fathoms. This is a corroboration which could scarcely be hoped for, and confirms the rate of decrease given by our theory. I may add further, that it is on this very rate, and the equability of it, that our barometric theorem depends, the agreement of which with the best observations that have been made, we shall presently witness.

PROPOSITION V.

A body of a given temperature, being elevated a certain height in the air, it is required to find its apparent temperature at the surface of the earth, excluding radiation or loss of temperature from lateral dispersion or otherwise.

Though the air receives its temperature by direct communication from the body it surrounds, and it becomes less and less by the body's attraction at greater distances, yet if any other body be placed in this atmosphere, of a temperature different from that of the air at the place in which it is, the surrounding air will derive a temperature from this body in the same manner as it does from the principal body. In the present instance we shall disregard the effect of attraction in the secondary body. We shall likewise confine ourselves to the influence on the air immediately beneath the secondary body, and suppose it not scattered over a greater portion of air.

Let us, in the first instance, suppose the attraction of the body uniform throughout the atmosphere, and since by (9) $V = t\sqrt{gbm}$, in which t the true temperature is unity at 32° Fahr., if τ be the true temperature of the body in the atmosphere, and τ_1 the true temperature which the surface of the principal body should have to give the temperature τ at the altitude x of the body,

$$6(\mathrm{V}^2 - V^2) = 6\,gbm\,(\tau_1^{\,2} - \tau^2) = gx, \text{ and}$$

$$\tau_1^{\,2} = \tau^2 + \frac{x}{6bm},$$

the square of the apparent temperature of the secondary body at the surface of the principal.

If the law of attraction of the principal body be $f'(x+r)$, its radius being r,

$$\tau_1 = \sqrt{\tau^2 + \frac{1}{6bm}\{f(x+r) - fr\}} \quad ..(17)$$

by formula (4).

Cor. Having found the value of τ_1 the effect of the secondary body on the surface of the principal is $\frac{1}{2}(\tau_1 + T) - T = \frac{1}{2}(\tau_1 - T)$; and by so much will it raise the true temperature of the air at the surface of the principal body, if the conditions of the Proposition be fulfilled.

Scholium.

We see by this Proposition that if a body float in the atmosphere of a temperature different from the air about it, the effect will be felt very sensibly at the surface. If it happen to be of a lower temperature than the circumjacent air, it will diminish the temperature at the surface; if of a higher it will increase it, though the temperature of this secondary body be actually less than that of the principal.

Hence it is apparent, that a cloud occupying the zenith may often raise the temperature on the earth several degrees, though the cloud is probably colder than the earth. A cloud transiently in the zenith could not sensibly affect the temperature of any body of magnitude; but thin filamentous bodies, as grass, straw, &c. would, as Dr. Wells repeatedly observed, be quickly affected.

If we regard a large portion of cloud it is obvious that whatever may have been its temperature at first, whether above or below the surrounding air, it must be continually elevated by the incessant radiation of the earth. This being returned by communication to the subjacent air, will necessarily advance the temperature at the surface. Hence radiation, which tends perpetually to depress the temperature of the earth, is the cause in a cloudy sky of raising the heat of the air; and hence, too, the reason that an overcast atmosphere, in the night particularly, is generally so much warmer than a clear one. Probably the heat generated from that condensation, which is necessary for the production of clouds from the invisible vapour, has no inconsiderable effect in raising their temperature above that of the air in which they float. Something may likewise be allowed for radiation from the clouds; but I apprehend this is but trifling.

The considerations we have just been drawn into, naturally lead us to another phenomenon, which has been conceived to be of more difficult explanation. Dr. Wells has often remarked that, in a clear night the air, some feet high, instead of being colder, is many degrees warmer than the ground beneath. Cloudless nights tend to cool the ground fast, particularly if it be covered with thin bodies, as grass, &c., by the process of radiation. And though the heat is transmitted in a few minutes from the ground to the top of the air, yet by the gravitation of its particles, the latter must always be returning a temperature equivalent to what had been some minutes before communicated to it. Hence for an altitude, as great probably as that which corresponds to a difference of temperature equal to the difference of heat simultaneously received and returned, the temperature of the air exceeds that of the air under it. This will hold only while the temperature of the earth is sinking; and the difference will be greater the greater the rate of diminution. But within a few minutes after the earth has reached its minimum temperature, the decrease will follow the law we have laid down. In the day time, when the earth is heating, the contrary will take place. The temperature near the surface will diminish more rapidly than our formula indicates; and more rapidly near the earth than higher up. This, as we have before observed, has been noticed by philosophers.

The above was the first view I took of this phenomenon, and which I insert merely to show its plausibility, and how liable we are to be deceived by confining ourselves to general views. For, if we examine this subject mathematically, we shall find that the greatest difference of temperature between the earth and air, must be as nearly as possible at the surface; whereas Dr. Wells found the temperature to go on progressively increasing for at least several feet high.

A better and probably the only explanation that can be given is, that it arises from aerial communication. From the very constitution of the air, according to our theory, the temperature of any one spot in the air is the result of the temperatures of all surrounding places, each having its due weight according to its situation and distance. That place will have the greatest influence, other things being alike, which is the nearest. Agreeably to this view, if a particular spot be covered with a greater radiating material than those in the neighbourhood round it, in the night it will become much colder than they, and the air next above will be of the same temperature; but as we ascend in the air, the distances from all the places will become more and more on a par, the apparent phases of the places and their influences on the temperature more nearly equal, and consequently the temperature itself of the place will approach nearer to a mean of the temperatures of them all. If this mean temperature be above the temperature of the place beneath, the heat of the air, instead of diminishing, will consequently increase with the altitude, and possibly to some extent both in temperature and height. On these principles the temperatures, and the anomaly of temperature in question may remain steady for a long time, without that continual diminution which the former view requires, and which, as far as I know, does not take place.

The reverse would be the case in the daytime, or when the radiating bodies were receiving heat faster than they were giving it off. And the reverse would also hold good in the night over a surface of less radiating power, surrounded by others of much greater. I believe Dr. Wells's experiments were made chiefly over a plot of grass, which is one of the greatest radiators.

M. Humboldt has advanced an observation which seems to militate against one of the simplest and most obvious consequences of our theory. By this theory the fall of Fahr. or Cent. temperature is the same for the same place and altitude, whatever be the temperature of the surface. Therefore the difference between the summer and winter temperatures at any altitude must be the same as at the place immediately under it. M. Humboldt, however, observes, "that the higher we ascend above the level of the sea, and the further we advance from the equator, the greater is the difference between the temperatures of the different seasons." The latter part of his observation, that is, relative to the distance from the equator, is very natural; but the former is not only opposed, but directly reversed, by the researches of Mr. Atkinson (Trans. Astron. Society, vol. ii, p. 217), who has minutely examined the general tendency of experiments. He says, "the difference of temperature between summer and winter is the same at the height of 11,000 feet as it is at the surface of the earth;" and again, "that the difference of temperature between summer and winter begins to be *diminished,* although in a very slight degree, somewhere between 11,000 and 23,000 feet." A mean, as our theory is, between such authorities, cannot be far from truth.

SECT. VII.

LAWS OF PRESSURE IN ATMOSPHERES, OR SUPERINCUMBENT MEDIA.

PROPOSITION VI.

The same things being supposed with regard to the motions and other things as in Proposition I, it is required to find the laws of elasticity of atmospheric media.

Let g, as before, be the accelerative force, or the velocity that would be generated at the altitude x at the end of 1^s, and a be the intensity of attraction in the same place. Then if e be the elasticity of the air, $\frac{e}{a}$ will be the mass of matter pressing it, and $\frac{eg}{a}$ the motion which would be generated by e at the end of 1^s. But $2VM \times \frac{Vn^{\frac{1}{3}}}{2} \times n^{\frac{2}{3}} = V^2Mn$ is the motion that would be generated by the action of the particles at the end of 1^s. These two motions must be equal, and

$$\frac{e}{a} = \frac{V^2Mn}{g}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (18).$$

Again, $Mn^{\frac{2}{3}}$ is the mass of a single stratum of particles, and $aMn^{\frac{2}{3}}$ the force with which it tends towards the centre of the solid body. This, therefore, will be equal to $\dot{e}$, the increment of the elasticity for $\frac{1}{n^{\frac{1}{3}}}$ distance, or one particle's excursion deep. Hence, dividing this equation $\dot{e} = aMn^{\frac{2}{3}}$ by (18), we have

$$\frac{\dot{e}}{e} = \frac{g}{V^2 n^{\frac{1}{3}}} \quad . \; . \; . \; . \; . \; . \; . \quad (19).$$

But $\dot{e} : \frac{1}{n^{\frac{1}{3}}} :: de : -dx = \frac{de}{\dot{e} n^{\frac{1}{3}}}$. Whence

$$\frac{de}{e} = -\frac{gdx}{V^2},$$

and by (4)

$$\frac{de}{e} = -\frac{gdx}{\mathbf{V}^2 - \frac{1}{6}\int gdx} \quad . \; . \; . \; . \; . \quad (20);$$

Replacing g for fx. Then integrating, we have

$$e = Q \left\{ \mathbf{V}^2 - \frac{1}{6}\int gdx \right\}^6 \quad . \; . \; . \; . \; . \quad (21),$$

Q being the constant of integration. When $V = \mathbf{V}$ the integral under the vinculum $= 0$; therefore

$$\frac{e}{E} = \left\{ 1 - \frac{\int gdx}{6\mathbf{V}^2} \right\}^6 = \left\{ 1 - \frac{80\int gdx}{Gbm(F + 448)} \right\}^6 \; . \; . \; (22);$$

b being the barometric altitude, and m the ratio of the mass of mercury to that of the same volume of the air at the temperature of melting ice, and corresponding to such an accelerative gravity G, as one previously settles on.

This, therefore, is a general expression for the law of the elastic force, whatever be the intensity and law of attraction.

Cor. 1. If the attraction be uniform

$$\frac{e}{E}=\left\{1-\frac{80gx}{Gbm\,(F+448)}\right\}^{6},$$

$$\text{or } x=\frac{Gbm\,(F+448)}{80g}\cdot\left\{1-\left(\frac{e}{E}\right)^{\frac{1}{6}}\right\} \quad . \ . \ . \ (23),$$

the altitude in feet, yards, metres, toises, &c. according to the conventional denomination of b, G, g; for $\frac{e}{E}$ being a ratio, the denomination of its members is of no consequence.

Cor. 2. Other things being alike, the greater F is, that is, the greater the temperature at the surface, the greater is the altitude; and the difference in the altitude is directly proportional to the difference of the Fahr. temperature; which coincides with *Cor.* 6, Proposition I.

Cor. 3. But the altitude remaining the same, the greater F is the greater $\frac{e}{E}$ must be; that is, the greater the temperature at the surface the less must be the fall of the barometer for the same elevation.

Cor. 4. Again, other things the same, the altitude x will be inversely as g, which is as the intensity of attraction; or, which is the same, inversely as the length of a pendulum beating seconds.

Cor. 5. Therefore if the gravity at the surface of the solid be not the same at all points in the air, the altitude, as computed for any one point, must be corrected for any other in the reciprocal ratio of the lengths of the seconds pendulums at the two places.

Cor. 6. And because the masses of two gases under the same elasticity and pressure are different, in the direct proportion to their specific gravities, while the mass of mercury remains the same, m, the ratio of the latter to the former, will vary in the inverse ratio of the said specific gravities. Therefore, under the same circumstances in other things, the altitude is reciprocally proportional to the specific gravities of the airs composing the atmospheres.

Cor. 7. If g be equal to the conventional G, the factor $\frac{bm}{80}$ is given by (11), and (23) becomes

$$x = 108{\cdot}7038562(F + 448) . \left\{1 - \left(\frac{e}{E}\right)^{\frac{1}{6}}\right\} \text{Eng. yds.} \ldots (24)$$

in a perfectly dry air.

Cor. 8. If $\int g dx = G \int \frac{r^2 dx}{(r+x)^2} = G \frac{rx}{r+x}$ and therefore, as in (13) and (14), if A be the altitude on the hypothesis of a uniform gravity, $x = \frac{rA}{r - A}$ is the altitude when the gravity is in the duplicate reciprocal ratio of the distances. Hence, Table 3, which gives the correction of altitude for the temperature, gives likewise the correction of altitude for the barometric altitudes.

PROPOSITION VII.

In every atmosphere not disturbed by any local or extraneous circumstances, the elasticities are as the 12th powers of the corresponding true temperatures, or the 6th powers of the Fahr. or Cent. temperatures, reckoned from the absolute zero.

For by (4) $6(\mathbf{V}^2 - V^2) = \int g dx$, which substituted in (22) gives

$$\frac{e}{E} = \frac{V^{12}}{\mathbf{V}^{12}} = \frac{t^{12}}{T^{12}} = \left(\frac{f+448}{F+448}\right)^6 \quad . \; . \; . \; (25).$$

Cor. 1. Hence the numeratom, or what is commonly called the density of the air, varies as the tenth power of the true temperatures, or fifth of the total Fahr. For $t^2 n$ being as e which is as t^{12}, n is as t^{10} or

$$\frac{n}{N} = \left(\frac{t}{T}\right)^{10} = \left(\frac{f+448}{F+448}\right)^5 = \left(\frac{e}{E}\right)^{\frac{5}{6}} \quad . \; . \; . \; (26).$$

Cor. 2. Therefore in (24), putting $\left(\frac{n}{N}\right)^{\frac{1}{5}}$ for $\left(\frac{e}{E}\right)^{\frac{1}{6}}$, we have this other equation for determining the altitude on the hypothesis of a uniform gravity, namely,

$$x = 108{\cdot}7038562\,(F+448)\left\{1-\left(\frac{n}{N}\right)^{\frac{1}{5}}\right\}\text{Eng. yds.}\ldots(27)$$

in which merely the density, as it is called, is used without the barometric pressure.

PROPOSITION VIII.

Because it generally happens that local or accidental circumstances disturb the natural depression of temperature, so as to render the formula comprehending only one temperature incorrect or insufficient, it is required to find a formula which shall include both temperatures, the upper and lower.

Since by (25) $\dfrac{f+448}{F+448} = \left(\dfrac{e}{E}\right)^{\frac{1}{6}}$

$$\left(\frac{E}{e}\right)^{\frac{1}{6}} - \left(\frac{e}{E}\right)^{\frac{1}{6}} = \frac{F+448}{f+448} - \frac{f+448}{F+448} =$$

$$\frac{(F-f)\cdot(F+f+896)}{(f+448)\,.\,(F+448)};$$

or putting F_1, f_1 for the absolute or total Fahr. temperatures, we have

$$a\left\{\left(\frac{E}{e}\right)^{\frac{1}{6}} - \left(\frac{e}{E}\right)^{\frac{1}{6}}\right\}\frac{F_1 f_1}{F_1+f_1} = a(F_1-f_1) = a(F-f) = A$$

by (11); in which a is the numeral coefficient 108·7038562, and A is the altitude on the supposition that the temperatures sink according to the law we have investigated. But if F_1, f_1 change, A will change to some other value x, though E, e remain the same. Let us suppose these temperatures change to F_2, f_2, then we have

$$a\left\{\left(\frac{E}{e}\right)^{\frac{1}{6}} - \left(\frac{e}{E}\right)^{\frac{1}{6}}\right\} \cdot \frac{F_2 f_2}{F_2+f_2} =$$

$$\cdot \quad \frac{F_1+f_1}{F_1 f_1} \cdot \frac{F_2 f_2}{F_2+f_2} A = x \quad . \quad . \qquad (28),$$

a formula in which the upper and lower temperatures, when they do not follow the mathematical law of an undisturbed atmosphere, are comprehended.

If we regard the equation in (28) between A and x, either the two lower F_1, F_2 or the two upper temperatures f_1, f_2 are equal, because there must be some common point to start from. Making the two lower and afterwards the two upper equal,

$$x = \frac{F_1+f_1}{F_1+f_2} \cdot \frac{f_2}{f_1} A, \text{ or } = \frac{F_1+f_1}{F_2+f_1} \cdot \frac{F_2}{F_1} A \quad . \quad . \quad (29),$$

equations giving the relation between the theoretic and experimental altitudes.

Cor. 1. If in the former expression f_2 be less than f_1, that is, if the observed temperature diminishes faster than the theoretic, x is less than A, or the real altitude is less than that which would be given from the lower temperature alone. On the contrary, if f_2 exceed f_1, or the temperature decreases slower than it should by theory, x is greater than A. The same conclusions follow from the second equation.

Cor. 2. Should we have computed the altitude by the lower temperature alone, we may from this deduce the altitude as given by the higher temperature, without again going through the most troublesome part of the process, especially if we use the tables whose construction will be given hereafter. For instance, if y be the altitude as given by Table 2 for the value of $\frac{e}{E}$ without any other correction, it corresponds to a lower temperature of 52° Fahr.—that at which this table is constructed. Therefore $\frac{y}{108{\cdot}7}$ is the thermometric depression due to y, and $500 - \frac{y}{108{\cdot}7}$ would be the total upper temperature in Fahr., on the supposition that 500 was the lower. Consequently, the altitudes being as these total Fahr. temperatures,

$$500 - \frac{y}{108{\cdot}7} : f_2 : : y : A_1 = \frac{y f_2}{500 - \frac{y}{108{\cdot}7}},$$

the true altitude by the upper temperature, which needs no farther correction, except for vapour, latitude, and variation of gravity.

Cor. 3. Since the temperature at each station must have an effect, and it is uncertain which ought to have the preference in the estimation of the altitude, we shall not probably be far out by assigning to each an equal influence, or by taking an arithmetical mean between the altitudes deduced from each separately. Now $\frac{\mathrm{y}}{108{\cdot}7}$ being the theoretic depression corresponding to a lower total temperature of 500, we have $500 : F_1 :: \mathrm{y} : A :: \frac{\mathrm{y}}{108{\cdot}7} : F_1 - f_1$, the theoretic depression corresponding to the lower temperature F_1. Whence $500 - \frac{\mathrm{y}}{108{\cdot}7} = \frac{500 f_1}{F_1}$; and therefore by the preceding *Cor.* $A_1 = \frac{\mathrm{y} f_2 F_1}{500 f_1}$. But as $\mathrm{y} : A :: 500 : F_1$, consequently $A_1 = \frac{A f_2}{f_1}$ and

$$\frac{A_1 + A}{2} = \frac{f_1 + f_2}{2 f_1} A = A + \frac{\Delta A}{2 f_1} \quad . \quad . \quad (30),$$

in which $\Delta = f_2 - f_1 = (F_1 - f_1) - (F_1 - f_2)$, the difference between the observed and theoretic depressions, the latter being calculated for the altitude A, and in the formula taken from the former.

The inferences drawn in *Cor.* 1 are here made somewhat more manifest.

If we set out with making the upper temperature common, that is, $f_1 = f_2$, the lower theoretic temperature F_1 will no longer be equal to the observed F_2. Let A, as before, be the altitude corresponding to F_2 alone; then $500 : F_2 :: \mathrm{y} : A :: \frac{\mathrm{y}}{108{\cdot}7} : F_1 - f_2$, from which we obtain

$$500 - \frac{y}{108{\cdot}7} = \frac{500(F_2 - F_1 + f_2)}{F_2} \text{ and } y = \frac{500\,A}{F_2}.$$

Substituting these quantities in the equation giving the value of A_1 in *Cor.* 2, we obtain

$$\frac{A_1 + A}{2} = \frac{A}{2}\left\{\frac{f_2}{F_2 - F_1 + f_2} + 1\right\} =$$

$$A + \frac{A(F_2 - F_1)}{2(F_2 - F_1 + f_2)} \quad . \quad . \quad (31).$$

These expressions (30) and (31) are equal, but the advantage of simplicity is obviously on the side of the former.

Our present expressions will not give precisely the altitudes of (28) or (29), but the difference will almost ever be exceedingly trifling; never, perhaps, amounting to what could be appreciated by observations. As both altitudes attempt to include local and accidental anomalies, it is difficult to decide to which the preference is due; but the latter being grounded on preserving the form of the function, has probably an advantage over the former, which is bottomed on the hypothesis of an equal influence in both temperatures.

PROPOSITION IX.

It is required to deduce approximate formulæ from the general.

1st. Calling F the total Fahr. temperature at the lower station and Δ the difference of these temperatures at the two stations, by (25)

$$\frac{E}{e}-\frac{e}{E}=\frac{F^6}{(F-\Delta)^6}-\frac{(F-\Delta)^6}{F^6}=\frac{12\Delta}{F}$$

when Δ is very small. Now if the Fahr. temperature be 32°, 56°, and 92° respectively, F will be 480, 504, and 540, and we shall have the three following equations:

$$\Delta=40\left(\frac{E}{e}-\frac{e}{E}\right),\ \Delta=42\left(\frac{E}{e}-\frac{e}{E}\right),$$

and
$$\Delta=45\left(\frac{E}{e}-\frac{e}{E}\right)$$

the last of these is identical with Mr. Leslie's equation between the barometric heights and depression of temperature.

No theorem of this kind can, for obvious reasons, be very correct, yet for small altitudes, such as the author intended his for, it may be sufficiently so, at least for all practical purposes, while its neat simplicity is a strong recommendation.

If x be the altitude in yards $x=108{\cdot}7\,\Delta$ by (11) and

$$x=\frac{108{\cdot}7}{12}F\left(\frac{E}{e}-\frac{e}{E}\right)=9{\cdot}06\,F\left(\frac{E}{e}-\frac{e}{E}\right)\text{nearly} \quad . \; . \; (32)$$

a theorem for small altitudes founded on the former; but probably not of very great accuracy in consequence of the upper temperature not being included.

If f be the total Fahr. temperature at the upper station, and the preceding expression be multiplied by $\frac{2F}{F+f}$ which, when f is very nearly F, it will become

$$x = 18{\cdot}02\,\frac{F^2}{F+f}\left(\frac{E}{e}-\frac{e}{E}\right) \quad . \quad . \quad . \quad . \quad (33)$$

including both temperatures.

2d. Because $\left(\frac{E}{e}\right)^{\frac{1}{6}} = \left\{\log^{-1}\log\frac{E}{e}\right\}^{\frac{1}{6}} = N^{\frac{1}{6}\log\frac{E}{e}}$,

in which N is the number whose hyperbolic logarithm is unity, and $\left(\frac{e}{E}\right)^{\frac{1}{6}} = \left(\frac{E}{e}\right)^{-\frac{1}{6}} = N^{-\frac{1}{6}\log\frac{E}{e}}$

$$\left(\frac{E}{e}\right)^{\frac{1}{6}} - \left(\frac{e}{E}\right)^{\frac{1}{6}} = \frac{1}{3}\log\frac{E}{e} + \frac{1}{3\cdot6^3}\left(\log\frac{E}{e}\right)^3 +$$

$$\frac{1}{3.4.5.6^3}\left(\log\frac{E}{e}\right)^5 + \cdot \quad . \quad . \quad .$$

But since $\frac{E}{e}$ seldom or ever in barometric admeasurements exceeds 2, $\log\frac{E}{e}$ seldom or never exceeds $\cdot30103$; nor does $\frac{1}{6^3}\left(\log\frac{E}{e}\right)^3$ seldom or ever, therefore, exceed $\frac{(\cdot30103)^3}{216} = \cdot000126$, or 1¼ yards in ten thousand. The succeeding terms are much less. All terms, therefore, after the first may be omitted as inconsiderable; and hence (28) gives

$$x=\frac{aF_2 f_2}{F_2+f_2}\cdot\frac{\log\frac{E}{e}}{3}=$$

$$36{\cdot}2346\,\frac{F_2 f_2}{F_2+f_2}\log\frac{E}{e}\quad .\ .\ .\ .\quad (34),$$

in which the log. is hyperbolic If we multiply the log. by the modulus ·434294482, and divide the numeral coefficient by it, we shall retain the same value to the expression, and change the hyperbolic into common logs. giving

$$x=83{\cdot}43325\,\frac{F_2 f_2}{F_2+f_2}\log\frac{E}{e}\quad .\ .\ .\ .\quad (35),$$

an approximation which can never differ sensibly from (28), and which will be found much more convenient where logs. are at hand, or where there are no tables computed.

This expression may be put under another form, which in most cases will be better adapted to calculation though probably not quite so correct. If we put $F_2=p+F$ and $f=p+f$, p being some fixed total temperature from which we choose to start, then

$$\frac{F_2 f_2}{F_2+f_2}=\frac{p^2+p(F+f)+Ff}{2p+F+f}=$$

$$\frac{p}{2}+\frac{F+f}{4}-\frac{(F-f)^2}{8p+4(F+f)}.$$

Therefore, if $F-f$ be very small in comparison of its denominator, this number of the thermometric factor will be inconsiderable, and may be neglected. Hence (35) will be nearly

$$x = 83{\cdot}43325\,\frac{p}{2}\left\{1+\frac{F+f}{2p}\right\}\log\frac{E}{e}\quad\ldots(36),$$

in which p may correspond to any degree of Fahr. we please. Supposing p corresponds to 52° Fahr. it is equal to 500, and (36) is

$$x = 20858{\cdot}3125\left\{1+\frac{F+f}{1000}\right\}\log\frac{E}{e}\ \ldots(37);$$

and if it corresponds to 32° Fahr. $p=480$, which makes the formula

$$\left.\begin{aligned} x &= 20023{\cdot}98\left\{1+\frac{F+f}{960}\right\}\log\frac{E}{e} \\ \text{or}\quad x &= 20023{\cdot}98\left\{1+\frac{C+c}{53\dot{3}}\right\}\log\frac{E}{e}\end{aligned}\right\}\ (38);$$

if the Cent. thermometer be used, the $\dot{3}$ being a repetend.

These last formulæ are evidently less correct than (35), by reason of the term omitted. On this account the formulæ generally give results too high. We may very nearly estimate the magnitude of the error thus committed. Supposing $\frac{E}{e}=2$, which corresponds to an altitude of about 6000 yards, $F-f=55°$ in round numbers. If, therefore, we suppose $F=-f$ and that $p=500$, the term omitted, since $F+f=0$, is

$$\frac{(F-f)^2}{4\,p^2}=\frac{55^2}{4\times 500^2}=\frac{11^2}{4\times 100^2}=\frac{121}{40000}=\cdot 003025,$$

or about 3 yards per thousand; and by so much would the formulæ (38) be too high. Were $p=480$ corresponding to 32° Fahr. it would be $\frac{55^2}{4\times 480^2}=\frac{121}{4\times 9216}=\cdot 003282$, or nearly 3·3 yards per 1000.

Again, if $\frac{e}{E}=\cdot 799$, which gives an altitude of about 2000 yards and a fall of temperature of $18^\circ{\cdot}4$ or nearly $18^\circ{\cdot}5$, then, if $F=-f$, when $p=500$ the error of omission is $\frac{18^2{\cdot}5}{4\times 500^2}=\frac{3^2{\cdot}7}{40000}=\frac{13{\cdot}69}{40000}=$ $\cdot 000342$, that is at the rate of about ⅓ yard per 1000. And if $p=480$ it is $\frac{18^2{\cdot}5}{4\times 480^2}=\frac{3^2{\cdot}7}{4\times 96^2}=$ $3{\cdot}4225\times\cdot 0001085=\cdot 000371$.

In formulæ (38) the error arising from omitting the term in question is insensible in altitudes of 2000 yards, being not above a foot per thousand, while if the altitude be only tripled, or 6000, the error is nearly tenfold for the same altitude, or more than three yards per thousand. Therefore, the error arising from neglecting this term does not become sensible unless in great heights, and then its effects increase most rapidly.

Laplace's formula, which coincides in form with (38), must, for the same reason, err in excess in very high altitudes. This formula, which, reduced to English yards, is

$$x = 20114{\cdot}9 \left\{1 + \frac{C + c}{500}\right\} \log \frac{E}{e} \quad . \quad . \quad . \quad (39),$$

differs, in the constant coefficient and in the denominator to $C + c$, from the one we have deduced from our general formula.

The difference in the constant coefficient we shall hereafter see, is the fault of our own formula, owing to its being computed on the hypothesis of an atmosphere totally free from vapour. It is, indeed, probable that the difference between these coefficients is not any ways equal to the allowance due to the vapour generally existing in the atmosphere, and particularly when the time of day, noon, for which Laplace's formula is said to be adapted, is considered. At this time the vapour is commonly in greater abundance than at any other time of the day; and the difference, therefore, between the state of the air and a perfectly dry air the greatest.

We may hence very easily account for the difference in the constant coefficients, and may justly credit Laplace's formula with a closer accordance with phenomena than our approximate one has. But the difference between the denominators is not so easily explained. Laplace himself says that he found his denominator agree better with experiments than that, namely, the one we have deduced, which is due to the expansion of gases. Nevertheless, M. Ramond, who has minutely compared the formula with the best observations, distinctly declares that it gives altitudes too high in summer, hot weather, and noon; and too low in winter, cold weather, and the mornings and evenings, which is obviously what might be expected from too low a denominator. Opposite and contradictory as these statements appear, they may, however, be both right. For, with respect to Laplace's observation, if we consider any one place, the numerator of the neglected term, that is, the difference of temperature, will be the same, while its denominator will increase with the temperature. The influence of this term, therefore, is less the greater the elevation of temperature, and when neglected, its apparent effect on the term retained would be, to require it to be greater, which is effected by diminishing the denominator as Laplace has done. Probably, however, Laplace, led away by the desire of simplifying the formula as much as possible, and of reducing the denominator to the round number $\frac{1}{500}$ or $\frac{2}{1000}$, has diminished it too much, so as to occasion the faults of which M. Ramond complains. In this way both may be right.

It is clear, from these circumstances, that the common logarithmic formula, though a good approximation, contains within it a source of error that may occasion anomalies which can with difficulty be estimated. Our (35) would, under all circumstances, probably be a much better approximation; at least, it has the advantage of greater theoretic accuracy.

3. Other formulæ of approximation may easily be found, which, under particular circumstances, may have their utility. Thus, if we reckon 30 yards for every 1-10th inch depression in the barometer, and denote the depression in tenths of an inch by x,

$$\frac{x}{4}+\frac{x}{4}.\frac{x-1}{2.3}=\frac{x.(x+5)}{2.3.4}$$

is a correction to be added to the preceding altitude in yards, which sum will agree very nearly with the theory in altitudes of less than 600 yards, when the lower temperature in Fahr. is 52°, and the loss of temperature agrees with the theory. If the lower temperature exceed or fall short of 52°, double the excess or defect, drawn into a thousandth of the preceding sum, will be a second correction additive with an excess, and subtractive with a defect. This will be the altitude when the loss of temperature is the same as theory gives, but if this be not the case add the theoretic loss by Table 1, corresponding with the altitude just found, to the upper temperature. which will give a new lower temperature. The mean of the altitude corresponding with this temperature and the altitude found before, will be the true altitude in a perfectly dry air.

Again, if $x=300\left\{1-\frac{e}{E}\right\}$ the altitude in yards corresponding with a lower temperature of 52° Fahr. is

$$30\tfrac{1}{4}\,x + x\,.\,\frac{x - 1}{2.3.4} \quad . \quad . \quad . \quad . \quad . \; (40),$$

which must be corrected for the excess or defect of the lower temperature, above or beneath 52° Fahr. and for any anomaly in the decrease of temperature, just as the preceding case.

This last approximation may be very useful in altitudes beneath 1000 yards. If the altitude be 1000 yards, it errs about three yards in defect; at higher altitudes it will be much more in error, and at lower much less; for at 560 yards it is only about one third yard too little. It may likewise be employed advantageously in much higher ranges than it is capable of compassing at once, by taking simultaneous observations at different heights, not exceeding 400 or 500 yards asunder, and adding them together. But as the tables we shall presently give are so very short and easy, it is but rarely that a person need have recourse to any other method; particularly as those who are engaged in such observations would, it is probable, be provided with them or with others.

Having now entered into the theory as far as it is needful, I shall proceed to compare it with experiments. The allowance for vapour, which is what probably can never be reduced to any certain rules, will be considered in the course of our comparison.

General Scholium.

CONSTRUCTION OF THE TABLES.

To avoid the trouble of recurring to formulæ, which, on account of the extraction of roots, and other tedious processes, was harassing and much exposed to errors, I determined to undertake the labour of computing tables by which the height might be more readily obtained. My object having been to unite the greatest possible brevity with a sufficient comprehensiveness, I was at first rather at a loss to know from which of the preceding theorems to make the computations. No. (28) presented the most accurate formula for practical purposes probably of either, but as the labour of embodying it in a table would have been exceedingly great, and the tables by no means concise, I was deterred from employing it. After some consideration I selected the very simple property offered by (25). By this theorem if Δ be the fall of Fahr. temperature, and we put $F = 52$, $1000\left\{1 - \left(\frac{e}{E}\right)^{\frac{1}{6}}\right\} = 2\Delta$, which substituted in (24) and A being taken $= \frac{108{\cdot}7\,\Delta}{1000}$ gives

$$x = \frac{2\,(F + 448)}{1000}\,A = A + \frac{2DA}{1000} \quad . \quad . \; (41),$$

supposing $D = 2\,(F - 52)$. Pursuing these principles I took the 6th roots of ·29 ·30 ·31 · · · · ·99 so many assumed values of $\frac{e}{E}$, and having subtracted them from unity, and multiplied the several remainders by 1000, with the halves of the products I took out the altitudes in col. 1 of Table 3, corresponding to the depressions in col. 11. The intermediate places were then filled up by interpolation, the computations being carried to thousandths of a yard, and the last figure rejected in forming the table, but with a due regard to its influence on the preceding figure.

In extracting the roots I used Hutton's large tables of logarithms, and took out the numbers to 7 figures, but, generally speaking, the last figure cannot be depended on.

Thus, Table 2 corresponds to a lower temperature of 52° Fahr., giving an altitude which we shall distinguish by the letter A. Table 3 is therefore made to correct it for any other temperature at the lower station, on the principle of the last term $\frac{2DA}{1000}$ of (41). It is merely a thousandth of the altitude A supposed to be given by Table 2, multiplied by twice the excess or defect of the lower temperature above or below 52°, and is plus in an excess, and minus in a defect.

The tables thus constructed it is clear would only correspond to a theoretic atmosphere, but the principle of Table 3 readily suggested a method of correcting for the anomalous decrease of temperature. For this purpose I added the 11th and 12th columns to Table 3, giving the Fahr. and Cent. falls agreeably to the altitudes in the first column. If the depression, therefore, be taken out according to the corrected altitude just found, and added to the upper temperature, it will give a new lower temperature, such as would very nearly agree theoretically with the upper. With this a new correction to the altitude A, previously found, may be had from Table 3, and half the sum of this and the preceding correction will be the true correction to be applied to A very nearly, according to *Cor.* 3, Proposition VII. Or we may subtract the given lower temperature from this new temperature, and multiplying a thousandth of A by it, apply the product, with its proper sign, to the first corrected altitude. This will in general be the shorter way.

Should the first and second corrected altitudes differ very materially, it will be necessary to take out a new fall of temperature with the second corrected altitude, and to repeat the rest of the process of the second correction. Or we may do thus,—find a fall of temperature corresponding to the difference of the first and second corrected altitudes, and, having multiplied a thousandth of A by it, add the product to the second corrected altitude if it exceed the first corrected altitude, or subtract it if this second corrected fall short of the first corrected altitude, and the sum or difference will give the true altitude extremely near.

In general this last correction will be very trifling. If A be 7000 yards, and the difference of the two corrected altitudes be 110 yards, it will amount to only 7 yards.

These computations are founded on the hypothesis of a uniform gravity, and are made for Paris. Table 4 will give the correction arising from the decrease of gravity, which is always additive; and Table 4 the correction arising from the ellipsoidal figure of the earth answering to the latitude of the place. It is usual to adapt tables of this kind to the latitude of 45° and level of the sea; but having M. Biot's measures of specific gravity and length of the pendulum for Paris at hand, I inadvertently proceeded so far with them, before I thought of it, in the construction of Table 2, which is the principal, that I did not like the labour nor indeed could I spare the time, for so many recomputations, particularly as it is not a matter of consequence for what place the tables are made, as long as we know the place.

There is, however, another circumstance somewhat affecting the accuracy of Table 2, which it is needful to notice, though it is but of small moment, and in practice quite insensible. I allude to this table having been computed on the assumption that the altitude is exactly 108·7 yards for 1° Fahr. of depression, whereas by M. Biot's experiments it is 108·7038562 yards. In 11,000 yards this would make not 2-5ths of a yard in defect, a matter utterly beneath our notice.

Allowance for Vapour.

The theorems which we have investigated, and the above rules for calculating the altitude, presuppose an atmosphere perfectly free from vapour. But as this is a state which can never take place, we are under a necessity of endeavouring to make such an allowance as may suit the effect which the vapour may have. This, however, is not so easily accomplished in a general way, at least theoretically, and to adapt the allowance to every contingency is probably impossible. The capricious manner in which vapour seems to exist at great distances from the surface, defies the control of everything in the shape of mathematical laws. Near the surface the same degree of humidity commonly prevails through a considerable extent; but, in the higher regions, it is not unusual for a patch of air, almost devoid of vapour, abruptly to be followed by one completely saturated; and the contrary. So that what prevails at the surface is no guide as to what occurs high up in the atmosphere. We cannot, therefore, from one estimate the other; nor reduce to any regular laws the effects of phenomena so destitute of order. Did vapour exist in the atmosphere with any degree of regularity, from knowing the temperature and quantity at the surface we might compute the height of complete saturation, and the proportional quantities at other heights. Amidst this uncertainty the best plan seems to be, either to make one general allowance for the mean quantity of vapour, and disregard the slight deviations from it which commonly occur, as Laplace has done, or to observe the tensions of the vapour with the heights of the barometer and thermometer at the two stations, and take a mean between the effects on the altitude. The former is the method I shall be obliged to pursue for want of observations, but the latter I think preferable, though it is not without its objections, particularly on account of the great horizontal distance which almost ever occurs between the places of observation.

To assign, from theory alone, the exact allowance which ought to be made for vapour, is obviously beyond our means, but we may approximate towards it.

Thus the range within which our observations are usually made, is from 30° to 90° Fahr.; and if we take the mean of all the tensions of vapour corresponding with 30°, 40°, 50°, 60°, 70°, 80°, and 90° Fahr., the principal points of this range, we have

$$\frac{\cdot20+\cdot28+\cdot40+\cdot56+\cdot77+1\cdot06+1\cdot43}{7}=\frac{4\cdot70}{7}=$$

·67, the mean tension of the whole range, nearly. But as it is seldom, except in winter, that the vapour of the atmosphere is at its full tension, we should probably not err much in taking 2-3ds of this ·67 for the mean elasticity of the vapour commonly in the air, or ·45. Dividing this by 30, the mean height of the barometer, and multiplying by 5-8ths, the specific gravity of vapour, that of air being unity, there results $\frac{45}{3000} \times \frac{5}{8} = \frac{1}{107}$, nearly, for the mean weight of vapour when that of air is unity.

Now by *Cor.* 6, Proposition VI, the altitude, other things being alike, is inversely proportional to the specific gravities of the atmospheric media. Therefore the altitude in the mixed air would be about 1 in 106 greater than in dry air, or 9½ per thousand, with the same temperature, and barometric ratio.

If, instead of taking the mean of the whole range, we compute from the tension of the mean temperature, 60° Fahr., it will be $\cdot 56 \times \frac{2}{3} \times \frac{1}{30} \times \frac{5}{8} = \frac{1}{129}$ nearly, or 1 in 128; that is, about 7¾ per 1000. The mean of these two estimates is nearly 8·5, or, in round numbers, 8 per 1000. So much, at a medium, we should allow for the effects of vapour; we shall shortly see how far this tallies with experience.

Eight yards per thousand would seem, at first sight, to be rather a large allowance; it is, however, scarcely greater than is given by the experiments of many philosophers, even at lower temperatures. Thus Dr. Moll, when making his observations to determine the velocity of sound, observed the tension of vapour to be about a 1-104th at 46°·4, a 1-81th at 52°·1, a 1-89th at 52°·2, and a 1-79th at 53°·7, Fahr. The relative weights of vapour which these forces give, are a 1-166th, 1-129th, 1-142th, and a 1-126th, that of the air being 1; that is, very little different from that assumed above, as a mean for a much higher temperature.

I shall now give the type or mode of calculation of some examples by which the use of the tables will be much better illustrated than it could by any description.

Example 1. According to M. Humboldt, the height of the barometer on Chimborazo was 167·20 lines, while at the South Sea it was 337·79 ; the attached and detached thermometers at the former station being + 10°, and — 1°·6 Cent., or 50° and 29°·12 Fahr., and at the latter 25°·3 Cent., or 77°·54 Fahr. The log of $\frac{E}{e}$ corrected, M. Ramond makes ·3041845 ; whence $\frac{e}{E}=·49638$. Therefore

		Yards.
By Table 2,	·49638 gives $A=$	5988·3
and „ 3,	77°·54 — 52° = 25°·54 „ +	305·9
		6294·2
Correction for gravity Table 4		+ 5·7
„ „ vapour	6·3 × 8 =	+ 50·4
„ „ for latitude tab.		+ 18·2
Corresponding dep^{n}. by Table 3,	58°·59	6368·5
	29 ·12	
	87 ·71	
	77 ·54	
·001 A ×	+ 10 ·17 =	+ 60·9
Altitudes . .		6429·4

This 60·9 correction must in every case be calculated for the first found altitude once corrected, that is, in the present case, 6·294. It would then be $6{\cdot}294 \times 10{\cdot}17 = 64{\cdot}0$.

This is the first example given by M. Ramond, from whose work, 'Sur la Formule Barométrique,' the data are extracted. By his "exact calculation" he makes it 5879 metres, which, multiplied by 1·093617, gives 6429·4 yds., exactly coinciding with the above. Had we allowed for the effect of the 60°·9 yds. correction, it would have made 0°·56 depression more, which would have augmented the altitude about 3 yds. The correction for latitude is taken from M. Ramond. Thus there is a most remarkable agreement between the two methods, considering the great altitude. I am not aware what the trigonometric altitude of this mountain is.

Let us now take M. Ramond's.

Example 2. La Barraque (Puy de Dôme), above Clermont-Ferrand. The barometers were 734·72 and 703·84 millimetres; the attached thermometers 21°·9 and 25°·6 Cent., and detached 23°·6 and 21°·8 Cent., or 74°·48 and 71°·24 Fahr. The corrected $\log \frac{E}{e} = {\cdot}0189448$, and therefore $\frac{e}{E} = {\cdot}95732$.

	Yards.
Hence ·95732 gives $A =$	393·7
$74^\circ{\cdot}48 - 52^\circ = 22^\circ{\cdot}48, \times {\cdot}002\,A =$	$+$ 17·7
	411·4
Correction for gravity . . .	
„ „ vapour . .	3·3
	414·7

Corresponding depression	3°·8	414·7
	71·2	
	75·0	
	74·5	
	+ ·5 × ·001 A =	+ ·2
Altitude . .		414·9

Ramond has computed this altitude at 379·9 metres. By levelling it was found to be 380·30 metres, or 415·9 yds., that is, 1 yard above our computation.

Example 3. Pic du Midi, above Tarbes. (Ramond, p. 235.) Barometers corrected 327·66 and 241·44 mm.; detached thermometers 20°·3 and 8°·3 Reaumur, or 77°·675 and 50°·675 Fahr.; and $\log \frac{E}{e} = \cdot 1326142$. Whence $\frac{e}{E} = \cdot 73686$.

By Table 2	A =	2696·9 yards.
77·675 — 52 = 25·675 . .		+ 138·5
		2835·4
		+ 1·1
Gravity . .		
Vapour . .		+ 22·9
Depression	26°·3	2859·4
	50 ·67	
	76 ·97	
	77 ·67	
	— ·70 . .	— 2·0
Altitude . .		2857·4

M. Ramond computes this at 2859 yards, and the trigonometric altitude is 2857·8 yards, that is 2-5th yard above our computation. No allowance has been made for the latitude, otherwise as Tarbes is in 43¼° N. latitude, the computation would have been a trifle more.

Example 4. Puy de Dôme, above Clermont-Ferrand, (idem, p. 235.) The two barometers being given in different measures, I shall merely extract the corrected log $\frac{E}{e} = \cdot0547056$, and the detached temperatures 18°·06 and 11°·7 Cent., or 65°·48 and 53°·6 Fahr.

$\frac{e}{E} = \cdot88165$ gives $A =$	1129·1 yds.
65·48 — 52 = 13·48 ,,	+ 30·4
	1159·5
Gravity . .	+ ·2
Vapour . .	+ 9·3
Corresponding depression 10°·8	1169·0
53 ·1	
63 ·9	
65 ·5	
— 1 ·6 . .	— 1·8
Altitude . .	1167·2

M. Ramond computes this at 1166·5 yards and in his tables states it at 1066 metres, or 1165·8 yds. Whether this be a trigonometric determination or not I cannot say; but if there be any it is.

Example 5. Pont du Berger (Puy de Dôme), above Clermont-Ferrand, (idem, p. 236.) Corrected barometers 733·55 and 689·45; $\log \frac{E}{e} = \cdot 0269269$; detached thermometers + 0°·3 and — 2°·9 Cent., or 32°·54 and 26°·78 Fahr.

$\frac{e}{E} = \cdot 93988$ gives $A =$ 558·7 yds.

32°·54 — 52° = — 19°·46 — 21·7

537·0

Vapour . . + 4·2

541·2

Corresponding depression 5°

26 ·8

31·8

32·5

— 0·7 . . — ·4

Altitude . . 540·8

M. Ramond computes this altitude at 538·4 yards, and the altitude by levelling was found to be 540·0 yards, or 0·8 yard beneath ours.

Example 6. Prudelles, east summit (Puy de Dôme), above Clermont-Ferrand, (idem, p. 236.) Corrected barometers 728·93 and 705·65; $\log \frac{E}{e} = \cdot 0140965$; detached temperatures 28°·3 and 25°·5 Cent., or 82°·94 and 77°·90 Fahr.

$\frac{e}{E} = \cdot 96806$ gives $A =$ 293·2 yds.

82·94 — 52 = 30·94 . . + 18·2

311·4

Vapour . . + 2·5

313·9

Corresponding depression	2·9	313·9
	77·9	
	80·8	
	82·9	
	— 2·1	— ·6
Altitude . .		313·3

M. Ramond computes this at 313·9 yards, the same as the altitude obtained by levelling, or 0·6 yd. above ours.

Example 7. Mont Perdu, above Barèges, (idem, p. 237.) Barometers 660·94 and 512·39; corrected $\log \frac{E}{e} = \cdot 1105613$; detached thermometers 20°·7 and 7°·5 Cent., or 68°·0 and 45°·5 Fahr.

$\frac{e}{E} = \cdot 77524$ gives $A =$		2257·8 yds.
68·0 — 52 = 16 . .		+ 72·3
		2330·1
Gravity . .		·8
Vapour . .		18·1
Corresponding depression	21°·6	2349·0
	45·5	
	67·1	
	68·0	
	— ·9 . .	— 2·0
Altitude . .		2347·0

M. Ramond's computation of this altitude is 2370·0 yards, while the tabulated number is 2353·5 which, if any, is a trigonometrical altitude. This is 6·5 yards above our computation, and no less than 16·5 yards beneath his. I cannot, therefore, help thinking that M. Ramond has committed some mistake; there cannot be this difference between his computation and ours.

Example 8. Gay-Lussac's great ascent, (idem, p. 237.) Barometers uncorrected 765·68 and 328·80 lines; corrected $\log \frac{E}{e} = \cdot 3638891$; thermometers 30°·8 and — 9°·5 Cent., or 87°·44 and 14°·90 Fahr.

$\frac{e}{E} = \cdot 43262$ giving $A =$		7083·8 yds.
87·44 — 52 = 35·44 . .		+ 502·1
		7585·9
Gravity . .		+ 8·3
Vapour . .		+ 60·7
Corresponding depression	70·43	7654·9
	14·90	
	85·33	
	87·44	
	—2·11	— 14·9
Altitude . .		7640·0

Were this corrected again for the effect of the 14·9 yards, it would reduce the altitude to 7639 yds., or 6·3 yards only above M. Ramond's calculation.

This is a very singular agreement in so great an altitude, and what could not have been anticipated. Such a coincidence between two methods so dissimilar may be relied on as giving a very near approximation to this extraordinary elevation.

In this experiment it is stated (Mr. Ivory's Philosophical Magazine for August 1825, p. 88) that the density of the air was reduced from 1 "to $\frac{1}{2}$ very exactly." Now this density corresponds to what we call the numeratom; and therefore by (26) $\left(\frac{e}{E}\right)^{\frac{5}{6}} = \frac{n}{N}$. But $\log \frac{e}{E} = \bar{1}\cdot6361109$, and 5-6ths of this is $\bar{1}\cdot6967591$, making $\frac{n}{N} = \frac{1}{2\cdot0102}$, which differs from $\frac{1}{2}$ only by $\frac{102}{40204} = \frac{1}{402}$ nearly. Thus the difference between the theory and observation is less than a 1-400th of the whole. Small as this difference is, it is very likely to be due to the presence of vapour in the lower strata of the air. Suppose, for instance, there be only a 1-150th in weight of vapour mixed with the air at the lower station, which, for so high a temperature as 87°, is a very small quantity, and that there be, as we might well believe, scarcely any in the upper. Then if this were taken away, and its place supplied with dry air, the density of the air instead of 1 would be $\frac{251}{250}$, which would increase the ratio of the densities from $1 : \frac{1}{2}$, to 1 to $\frac{1}{2\cdot008}$, differing from the theoretic computation $\frac{1}{2\cdot0102}$ by a $\frac{1}{1835}$th of the whole, a quantity much within the limits on which the observations can be depended.

We have in this a very nice experimental corroboration of our Proposition VII, namely, that the pressures diminish as the 12th powers of the true temperatures.

Example 9. Etna, above Catana (Saussure's Voyages, § 941), from Ramond, page 238, $\log\frac{E}{e} = \cdot1718863$; detached thermometers 23°·1 and 4°·4 Cent., or 73°·58 and 39°·92 Fahr.

$\frac{e}{E} = \cdot67315$ giving $A =$		3469·5 yards.
73·58 — 52 = 21·58 . .		+ 149·7
		3619·2
Gravity . . .		+ 1·9
Vapour . . .		+ 29·0
Cor. depress.	33·58	3650·1
	39·92	
	73·50	
	73·58	
	— ·08	— ·3
Altitude . .		3649·8

Our computation exactly coincides with Ramond's, which is 1712·32 toises. We have not, that I know

of, any trigonometric altitude of Etna. Trembley found by his formula, and I suppose this observa-
toises. pouce.
tion, 1714 1. Dangos having taken a barometer to the top of Etna made the altitude with Trem-
toises. pouces.
bley's formula 1711 5; and Saussure computed it at 1713 toises.

These are all the computations which M. Ramond has given at length, and which for the sake of comparing the two methods together, and our theory with observation, I have likewise given at length. The close argument of the two rules and of ours with observation, through a series of altitudes extending from 300 yards to 7600, needs but little to be said. I shall now, therefore, compute one or two of those cases wherein the observed and theoretic depressions differ the most, for the purpose of comparing the approximative method we have pursued with the more rigid theorem (28). Wherever these depressions agree, it is obvious there can be no difference in the results of the two methods, and we need not trouble ourselves with the examination. In small altitudes too, even when the depressions are very unequal, the results cannot be far out. It is only in great altitudes that we can expect any sensible deviation.

In the first example the difference between the depressions is $10^\circ{\cdot}17$, and the altitude upwards of 6400 yards; so that here if there be any difference it must be very manifest. We have $\frac{1}{6}\log\frac{E}{e}$

$=\frac{\cdot 3041845}{6}=\cdot 0506974$ whose number is 1·12382 and its reciprocal ·88982. The difference ·23400 multiplied by 108·7038562, and $\frac{F_2 f_2}{F_2+f_2}=\frac{525{\cdot}54\times 477{\cdot}12}{1002{\cdot}\ 6}$ will give the altitude exclusive of the effects of vapour, gravity, and latitude. By logarithms we have

·234	log	$\bar{1}{\cdot}3692159$
108·7038562	,,	2·0362449
525·54	,,	2·7206058
477·12	,,	2·6786276
1002·66	log. arith.	$\bar{4}{\cdot}9988463$
	6361·2	3·8035405
Vapour	+ 50·9	
Gravity	+ 5·8	
Latitude	18·2	
Altitude	6436·1	

If our vapour in the approximate calculation had been increased agreeably to the true altitude, that is, if the effect of the correction for 60·9 yards had been allowed for, the altitude would have come out 6433 yards, being only three yards beneath the rigid calculation from (28). This is, perhaps, one of the extreme cases. Probably so great a deviation could not again be found, in any good observation, except at a much greater altitude. Let us try the great ascent of Lussac, our 8th Example.

Here $\frac{1}{6}\log\frac{E}{e}=\frac{\cdot 3638891}{6}=\cdot 0606482$. The number is 1·149861, its reciprocal, ·869665, and their difference ·280196; $F_2 = 535·44$ and $f_2 =$ 462·90.

·280196	. log .	$\bar{1}$·4474619	Altitude	7561·8
108·7	. . „ .	2·0362449	Vapour	+ 60·5
535·44	. . „ .	2·7287108	Gravity	8·3
462·90	. . „ .	2·6654872		7630·6 Alt.
998·34	log. arith. .	$\bar{3}$·0007215		
Alt. 7561·8		3·8786263		

This altitude is, therefore, nearly 8½ yards beneath what Example 8 would be a second time corrected.

Indeed, the higher the altitude the greater is the deviation, and in a much greater ratio. In very great heights the error is considerable, though still a long way within what the observations can be depended on. Thus if the lower temperature be 52°, and $\frac{e}{E} = ·35135$, the theoretic depression would be 80°, and altitude 8695·65 yards by the tables, and 8695·9 by the formula; the difference being attributable to the value before alluded to, 108·7 yards, which is made the foundation of the tables. But if the ratio and lower temperature remain, and the depression be changed first to 70° and then to 90°, the tabular altitudes will be 8789·6 and 8601·7 yards, while those by the formula are 8807·2 and 8582·1 yards; surpassing, in the first instance, the tables by 17·6 yards, and falling short, in the second, by 19·6 yards. Other things remaining, if $\frac{e}{E} = ·606369$, the altitude and depression will be only the halves of the preceding, or 4347·82 yards, and 40°. And if we now suppose the depression to change to 30°, and then to 50°, the tabular altitudes will be 4393 and 4302·6 yards, and the theorem altitudes 4396·7 and 4298·2 yards, being 3·7 above and 4·4 yards beneath the former. These differences are only between a fourth and a fifth of the former, though the deviations in the depressions are the same, and the mean altitude is reduced to only one half.

We see, from these calculations, that when the observed depressions are less than theory, the tabular altitudes come out too small, and too great when the depressions exceed the theory; but the errors are not quite so great in the former as in the latter case. We likewise perceive that these deviations diminish rapidly with the altitude, even faster than the squares of the altitude; so that it is only in the most delicate observations that they could at all become sensible. It is to me doubtful whether any means of instrumental altimetry at present in use could render them sensible.

It will be observed that M. Ramond's calculations, from Laplace's formula, are greater than ours by 2·1 yards in Lussac's ascent, and less by 6·7 yards in Humboldt's observation on Chimborazo. The former altitude exceeds the latter by 1200 yards, but the difference between the observed and theoretic depressions of temperature is only about a fifth in that to what it is in this. May not the inability of Laplace's formula to comprehend the fluctuations of temperature and the local affections, be the cause of many of the anomalies in the computed heights? No theorem, probably, can entirely neutralize the effects of locality and accident; but the chances are evidently more in favour of a complete than an approximative one. And we might observe, generally, that the altitudes are more to be depended on, the nearer the observed depression approaches the theoretic; for, at such times, the air is nearer its mean state than at others.

According to our own tables, and the common logarithmic formula, the change of a degree or more in the upper temperature produces the same effect on the altitude as an equal change in the lower; but it is not so in the theorem (28). Differentiating the thermometric factor, we find

$$\frac{f^2dF + F^2df}{(F+f)^2};$$

so that the effects of equal changes in the temperatures are in the duplicate inverse ratio of the total temperatures. The upper temperature has, therefore, greater influence over the altitude than the lower, and since the lower is more exposed to disturbance, this theorem must be more accurate than the common formula, or than our tables. Altitudes likewise, determined from the upper temperature alone, must in general be more correct than if determined from the lower temperature alone. To decide this, by an appeal to observation, would require our going through a long series of calculations; since in very good observations, where the quantity to be ascertained is but small, the unavoidable errors in the trigonometrical observations and in the barometric, would render a comparison with one, two, or half a dozen cases, by no means decisive or satisfactory. Having, however, shown that our (28) very correctly represents the mean state of phenomena, we may compare the separate results of the two temperatures with the result it gives, with the same confidence and safety as if we were to go through, and take the mean of comparisons, with a hundred actual experiments. For instance, in our first Example the altitude, from the lower temperature alone, is 6368·5 yards; and by *Cor.* 2, Proposition VII, the altitude, by the upper alone, is

$$\frac{A\,.\,(29{\cdot}12+448)}{500-\dfrac{A}{108{\cdot}7\,\ldots}}=\frac{5988{\cdot}3(29{\cdot}12+448)}{500-55{\cdot}09}=\left\{6421{\cdot}8\text{ yds.}\right.$$

or 6496·1, allowing for latitude, gravity, and vapour. But we have lately computed this altitude, by (28), to be 6436·1 yards, which is 67·6 yards above that by the lower temperature, and 60 yards beneath the calculation by the upper. The difference is $7\frac{1}{2}$ yards in favour of the upper temperature.

Having instituted a comparison between the theory and all the experiments calculated out by M. Ramond, we shall now compare the theory with those made by Sir George Shuckburgh, which are esteemed to be amongst the most exact that we have.

Two stations having been chosen at Mont Saleve, the upper barometer stood at 25·712, the attached thermometer at 78°, and the detached at 65°; the lower barometer at 28·399, the attached thermometer at 72°·1, and the detached at 73°·9. The altitude, by very careful trigonometrical operations, was found to be 943·8 yards.

	25·712		
By Table 1, — 6° at 25½ gives	— ·015		
	25·697	log	1·409883
	28·399	„	1·453303
	·90485		·956580

By Table 2 ·90485	gives	898·3 . . *A*
73·9 — 52 = 21·9	„	39·3
Cor. depress. 8°·62		937·6
65 + 8·62 — 73·9	„	— ·3
		937·3
Vapour =		7·5
Gravity		·1
Altitude		944·9 yards.

This altitude is 1·1 yard in excess. Two other observations were made at the same place which differed 2·3 and 3·6 yards in defect. A second set of observations were afterwards made on the Mole, another mountain whose altitude was geometrically determined at 1403·8 yards; and a third by a return to Mont Saleve, where the geometric height was accounted 943 yards. The following are the observations and computed altitudes by our tables, with the exception of an allowance for latitude.

Mont Saleve, first set; Geometric Altitude, 943·8 *yards.*
(Philosophical Transactions for 1777, p. 529.)

Lower Station.			Upper Station.			Compd. Alt. in English yards.	Diff. yards.
Barometer inches.	Temps. Fahr. Mercury.	Air.	Barometer inches.	Temperatures. Mercury.	Air.		
28·399	72°·1	73°·9	25·712	78°·0	65°·0	944·9	+1·1
28·394	71·6	73·0	25·7025	73·4	64·0	941·5	—2·3
28·3935	71·1	72·5	25·69	69·7	62·0	940·2	—3·6
						mean diff.	— 1·6
The Mole, second set; Geometric Altitude, 1403·8 *yards.* (Philosophical Transactions for 1777, p. 541.)							
28·1295	60·4	61·9	24·1437	57·0	54·8	1409·8	+6·0
28·130	60·4	61·8	24·142	56·9	56·0	1412·2	+8·4
28·132	60·9	63·0	24·167	56·0	56·0	1403·6	—0·2
28·136	61·8	63·9	24·178	57·2	56·0	1402·0	—1·8
28·135	62·4	64·0	24·184	59·6	57·0	1402·8	—1·0
28·131	62·6	63·6	24·190	61·0	57·0	1399·7	—4·1
						mean diff.	+ 1·2
Mont Saleve, third set; Geometrical Altitude, 943 *yards.* (Philosophical Transactions for 1777, p. 553.)							
28·404	58·1	58·8	25·6533	58·0	56·2	940·3	—2·7
28·404	58·5	60·8	25·655	56·2	58·0	942·1	—0·9
....	59·3	62·0	25·662	56·2	57·2	940·9	—2·1
......	59·3	62·2	25·660	56·4	57·4	942·1	—0·9
						mean diff.	—1·65

In the first and second sets the barometers are uncorrected; but in the last set I am ignorant whether they are corrected or not; I have computed as if they were, though, from the comparison of Shuckburgh's computations with mine, I am somewhat inclined to a contrary opinion.

A very singular, almost an extraordinary, coincidence here appears between the two sets of observations at Mont Saleve. They are both about $1\frac{1}{2}$ yard in defect and give means differing not two inches. The observations on the Mole are nearly as much the contrary way; so that if the one be set against the other, our tables represent most faithfully the observations of Sir George Shuckburgh.

The same volume of the Transactions contains the experiments of General Roy, on the mountains Schehallien and Snowden.

Schehallien South Observatory, Trigonometrical Altitude, 699·33 *yards.*
(Philosophical Transactions, 1777, p. 770.)

Lower Station.			Upper Station.			Compd. Alt. English yards.	Diff.
Barom. inches.	Temperatures. Merc.	Air.	Barom. inches.	Temperatures. Merc.	Air.		
29·643	58¾°	58°	27·432	48°	47¾°	699·2	— 0·1
West Summit of Schehallien, Trigonometrical Altitude, 1093·67 *yards.*							
29·595	59½	56	26·194	46	45	1099·3	+ 5·6
29·610	50¾	50¼	26·220	44	42	1190·0	— 3·7
						mean diff.	+ ·95
Snowden, Moel Eilio, Trigonometrical Altitude, 790·33 *yards.*							
*29·693	62½	62½	27·714	54	51	627·8	— 162·5
30·036	68	68¾	27·543	57	56	797·5	+ 7·2
30·027	69½	69½	27·533	58¼	57	798·8	+ 8·5
				mean diff. excluding first			+ 7·85
Snowden Peak, Trigonometrical Altitude, 1185 *yards.*							
30·154	56¾	56⅜	26·462	47½	45⅛	1181·2	— 3·8
30·165	60	60	26·468	49¼	47¼	1187·9	+ 2·9
30·140	61½	61¾	26·488	†60½	54	1192·4	+ 7·4
30·144	62	62	26·478	53¾	51	1187·1	+ 2·1
29·984	56½	55¼	26·271	42¾	43	1187·9	+ 2·9
29·978	58½	57¾	26·279	44	43½	1186·2	+ 1·2
29·972	60	60	26·280	44½	44¾	1187·6	+ 2·6
29·974	61½	61	26·280	44¾	45	1188·2	+ 3·2
29·976	62½	62	26·282	46½	46	1191·2	+ 6·2
						mean diff.	+ 2·75

* There seems to be an error in this observation somewhere—probably a typographical one.

† Ought this to be 50½°? The observation would be more consistent with the rest.

These experiments on the whole agree very well with theory. The first two sets differ not a yard from the trigonometrical computations, and the last set only 2¾ yards in 1200. At Moel Eilio the difference is by far the most. It amounts to 7¾ on about 800, or one per cent.; a difference, which, though not of very great magnitude, it is impossible to attribute to anything but local or accidental circumstances.

The following experiments by M. Ramond, extracted from his work 'Sur la Formule Barométrique,' are made with more attention to accuracy than probably any which have yet been published. M. Ramond, who assiduously devoted several years to barometric observations, informs us, that M. Cournon measured the heights by levelling upwards and downwards, and that each was ignorant of the other's results until after they had been exchanged.

La Barraque, a very favorable situation. Altitude by levelling 380·30 *metres, or* 415·91 *yards.*

Time. 1809.	Weather.	Lower Station.			Upper Station.			Alt. Yards.	Diff.
		Barom.	Temperatures. Merc.	Temperatures. Air.	Barom.	Temperatures. Merc.	Temperatures. Air.		
Mar. 12	N.E. a little strong	723·60	53°·42	32°·0	688·55	36°·5	29°·84	417·8	+ 1·9
Feb. 26	N.E. very fine	737·73	54·14	36·14	702·54	41	35·6	417·9	+ 2·0
	. . .	737·65	54·32	35·96	702·49	41·36	35·78	417·4	+ 1·5
Mar. 8	E.N.E. a troubled sky	738·55	57·38	49·28	704·85	50·72	47·66	416·7	+ 0·8
„ 1	S.W. a little strong	735·70	54·86	48·56	702·30	50·90	47·84	416·3	+ 0·4
„ 20	W. clouds .	731·90	56·30	52·34	699·39	61·16	51·98	415·0	— 0·9
„ 19	S.W. great clouds	729·55	57·20	54·86	697·09	59·90	53·60	417·4	+ 1·5
May 15	E.N.E. "superbe"	734·72	71·42	78·08	703·84	74·48	71·24	414·6	— 1·3
								mean diff.	+ 0·74

These observations are arranged according to the order of M. Ramond. Had they been arranged according to the lower temperatures of the air, the last column would have presented errors pointing out a decrease as the temperature of the air rises, and showing that the allowance for vapour is too much in the lowest temperatures and too little in the highest. It may, perhaps, be urged that the height being only 400 yards is too small to depend on the results, and that indications of this sort may easily be confounded with the inseparable errors of observation. Four or five times the height would doubtless have been better, but where so much care has been taken confidence in the results cannot be withheld, particularly if they are consistent, and the situation, as the above is acknowledged to be, is favorable. But the fact is, these errors will very nearly disappear if we come to calculate the allowance for vapour a little more nicely. For instance, our allowance is estimated at about 2-3ds. of the tension of vapour at 60°, that is, about ·45 inch, which on our present altitude amounts to 3·3 yards. Now at the temperature of 32° Fahr. the full tension at which the vapour often exists is ·2. If, therefore, we call it ·15 or ·18, each of the first three altitudes must be diminished about 2·0 yards, which would reduce the errors to — 0·1, ·0, and — 0·5. Again at 78° Fahr., which is the temperature of the last observation, the tension is about ·95 inch, or about 1-3d more than our assumed mean tension; and if we increase our altitude on this account by 1-3d of 3·3, it will exactly agree with the altitude obtained by levelling. Thus the mean of all would be reduced, if due allowance was made in each, to an exceedingly small quantity.

We shall add the testimony of another of M. Ramond's experiments at Vassan, a place which he describes as excellent for such kind of observations, and made under very favorable circumstances as to weather. Unfortunately he has given us but one observation at this place, but the temperature at which it was made, and its very close agreement with phenomena, are of a decisive character as to allowance for vapour. As another example, I shall likewise give the calculation at length. The upper barometer was 694·45 the lower 730·33, the difference of their temperatures 8°·82 Fahr. the upper barometer being of the lower temperature, the temperatures of the air were 14° Cent., or 57°·2 Fahr. at the higher station, and 18°·4 Cent. or 65°·12 Fahr. at the lower. The true ratio of the barometers is $\frac{694{\cdot}45}{730{\cdot}33} + \frac{694{\cdot}45}{730{\cdot}33} \times {\cdot}0001001 \times 8{\cdot}82$, and the type of calculation, thus

$$\begin{array}{llll}
694{\cdot}45 & \log & 2{\cdot}841641 & \\
730{\cdot}33 & \log & \underline{2{\cdot}863519} & \\
 & & \bar{1}{\cdot}978122 & {\cdot}95087 \\
{\cdot}0001001 & \log & \bar{4}{\cdot}000434 & \\
8{\cdot}82 & & \underline{{\cdot}945469} & \\
 & & \underline{\bar{4}{\cdot}924025} & \underline{{\cdot}00084} \\
 & & & {\cdot}95171 \\
 & & & \text{true ratio.}
\end{array}$$

True ratio ·95171 by tables		446·50 yards
65°·12 — 52° = 13°·12 gives		11·72
Corresponding temp.	4°·21	458·22
	57·20	
	61·41	
	65·12	
	— 3·71	— 1·66
		456·56
Vapour		3·65
Altitude		460·21

By levelling as before, M. Ramond gives the altitude equal to 420·80 metres, or 420·80 × 1·093616 = 460·18 English yards, that is only ·03 yards beneath our computation.

We may judge from the examples given of the great accuracy with which our tables represent such observations as can be depended on. There is, indeed, if we employ the theorem (28), only wanting some correct method of allowing for the vapour to give barometric altimetry its utmost perfection. But this cannot be effected without further experiments. All that has yet been done is little better than guessing.

M. Ramond, to whose experience in this sort of experiments the utmost deference is due, lays considerable stress on the hour at which the observations are made. The best time he conceives to be about noon, at which time he thinks the atmosphere is nearest a state of internal tranquillity. At other times, particularly if distant from noon, the errors of computation are great. Indeed, their amount is so considerable as to be almost incapable of explanation. Thus 51 observations, from October 14th to December 21st, gave the following results:

14	obs. at	7 hrs. a.m.	produced	a mean	of	251	metres
23	,,	noon	,,	,,	,,	258	,,
7	,,	3 p.m.	,,	,,	,,	254	,,
7	,,	9 p.m.	,,	,,	,,	252	,,

In the same way 99, from August 1st to November 23d, made

12	obs. at	6 a.m.	,,	,,	,,	915	,,
6	,,	8 a.m.	,,	,,	,,	924	,,
38	,,	noon	,,	,,	,,	936	,,
19	,,	4 p.m.	,,	,,	,,	921	,,
14	,,	10 p.m.	,,	,,	,,	907	,,

The second set of the second series, I expect, should be 16, otherwise there are only 89 instead of 99 observations.

Unluckily M. Ramond has not furnished us with the observations from which these altitudes are computed, so that we are in the dark in regard to particulars. Enough, however, is shown in these 150 observations to be conclusive of the influence of the hour, or of the temperature, or vapour, or whatever it might be. M. Ramond has added three other sets of observations in places stated to be less favorable to this sort of experiments, all converging towards the same point. They do not, considering the altitudes, present those great deficiencies which the preceding do; yet are they marked with sufficient consistency to claim one's confidence. That others may judge for themselves I subjoin them, lamenting again that the author has not supplied us with the particulars. The elevation of the higher station, Pic du Midi, above his calculation at Barèges, in the first two sets is stated at 1654 metres, which I presume, for it does not appear, is a trigonometrical or levelling altitude.

First set, August 30th, 1803:

At 10 hours a.m.,	1663	metres.	At noon	1677	metres
10¼ „ „	1669	„	0½ p.m.	1679	„
11 „ „	1672	„	1 p.m.	1674	„

Second set, same station, Sept. 15th:

At 10 a.m.,	1652	„	Noon,	1664	„
10½ „	1659	„	1 p.m.	1670	„
11 „	1662	„			

Third set, Sept. 12th; height above his calculation at Barèges, 832 metres:

At 10 a.m.,	846	„	Noon,	846	„
10½ „	846	„	1 p.m.	855	„
11 „	848	„			

It seems that at each of these places and hours there was only a single observation; at least, nothing to the contrary is implied.

With so many and such conclusive testimonies, one must subscribe to the very singular fact which M. Ramond has announced. But to what is it owing? Is it to the fluctuations of temperature and vapour, or to currents arising from an unequally heated surface disturbing the regular gradations of temperature and density, as M. Ramond conjectures? Such questions are difficult to answer. That the effects are too great for any possible errors in the logarithmic formula, owing to change of temperature and vapour, the slightest glance at the magnitude of the errors will convince any one; but they may be in part due to this and in part to M. Ramond's currents. Further experiments, however, are necessary before any conclusion can be come to; and, if the subject of barometric altimetry be worthy of attention, such experiments deserve to be made. At all events, until the cause of this anomaly be settled no progress can be made, and every result will be clouded with uncertainty.

Scholium 2.

The reader who has carefully perused the preceding Section, written about twenty years before the present time (December, 1846), would very reasonably conclude that the theory so faithfully represents phenomena as to leave nothing to be desired. So, indeed, I thought when I composed it, and until within these very few years. If I had doubt upon any part, it was upon the allowance for vapour. I had not the same confidence in that which I had in other portions of the section. It struck me that as that was empirical it was subject to a good deal of uncertainty; but as the agreement of the computations with phenomena, under this allowance, was so very close, it very much removed any doubts of its being wrong to any sensible extent. On the theory I had no apprehensions, nor on the constants with regard to the expansion of air and the relative specific gravities of air and mercury. The known skill and care of Dalton and Gay-Lussac, confirmed, as it afterwards was, by those able philosophers MM. Dulong and Petit, left my mind easy on the score of the expansion assigned by them to the atmosphere between 32° and 212° Fahr. Nor, from the high reputation of MM. Biot and Arago, had I the slightest misgiving as to the accuracy of the relative specific gravities of mercury and air, determined by those philosophers. The recent labours, however, of MM. Magnus and Regnault, by the different expansion they have given to the atmosphere, have thrown all these nice agreements between theory and experiment to the winds. They recall to my mind what an old African captain said to me just after Barlow's discovery of the effect of the ship's iron on the needle. "When I was at sea," he observed, "we trusted the compass as a true and faithful guide, and we were never deceived; but now science is further advanced, Barlow shows us it cannot be trusted at all, and I actually should be afraid to go to sea with it. I wish he had left us alone in our happy ignorance!"

We are inclined to utter the same wish with respect to the new experiments of MM. Regnault and Magnus. We almost dislike the benefits they have conferred.

According to their expansion, the point of absolute cold is 491°·132 Fahr. below the melting of ice, instead of 480°. So that if all other things remained the same, our theorem (23) and tables would give altitudes, when $F = 52°$, about 22 yards in a thousand too small, an error which we think impossible. If we had any doubt remaining in our mind on our theorems, the almost perfect coincidence of (26) with the experiment of Gay-Lussac's ascent would annihilate it. That theorem, deduced from the same Proposition which runs through and governs both this and the preceding Section, and being turned into a relation — quite, I presume, unexpected — between the numeratoms and elasticities, is found to accord almost exactly with the observations of Gay-Lussac, in one of the most severe tests we have yet been able to put it to. One may therefore rest satisfied that there is no defect in the general solution. To what, then, supposing MM. Magnus and Regnault's numbers right, can so great an excess as 2·2 per cent. be attributed? Supposing we omit the correction for vapour, we are then 1·4 per cent. too much by employing their coefficient. If we refer back to the general expression (23) any error can rest only on the value of m, if on anything—the ratio of the specific gravity of mercury to air; for in G and b no error can lie. Now, by MM. Biot and Arago, $m = 10463$. In the 'Annuaire' for 1835, and up to 1840, m was stated to be 10366, but is now reckoned 10462. That may have been a misprint, and this may be the supposed true number. To try whether any probable error in m could explain the anomaly, we made the following computation. We assumed the weight of water and mercury to be pretty truly defined, that is, that a cubic foot of water weighs 1000 oz. avoirdupois, or 62·5 lbs.; that 7000 grs. troy are equal to a lb. avoirdupois; and that mercury is 13·598 times heavier than water. Hence we calculate that the weight of a cubic foot of air, supposing mercury to be first 10462 times heavier and then 10262, is 568·64 grs. in the former case, and 579·73 in the latter. It requires, therefore, an error of 1·9 per cent. in the determination of the net weight of the air to reduce 10462 to 10262, which would bring the numerical values of the theorem and tables back to about their present accordance with experiments, and keep the new factor of expansion. But, according to the experiments of M. Regnault on 10 litres or 610 cubic inches of air, his extreme observations did not differ more than ·65 per cent. or one third of the amount. It hardly, therefore, seems likely that three times the error could be committed in any average of carefully conducted experiments.

Again, between the mean specific gravity of mercury 13·588597 and 13·59593, as determined by MM. Biot and Arago on the one side, and M. Regnault on the other, the difference of weight in a cubic foot of mercury is 3208·3 grains. Divide this by 10462, and it gives ·307 grains for the error on a cubic foot of air to produce the same relative specific gravities,—quite insufficient to account for the difference required in the specific gravity of air and mercury, to restore the theorem to its present accuracy, after introducing the coefficient of expansion of M. Regnault.

Unless, therefore, the carbonic acid gas in the atmosphere—of which no account has been taken—balance the effect of the aqueous vapour, I hardly see how we can reduce the difference between the tables and observation to reasonable limits; that is, so as to have the probable error of the specific gravities of air and mercury to make up the difference between the tables and observation. But here, again, we are beaten; for the quantity of carbonic acid gas is only 1 in 1000, while the vapour is 15 times as much. No solution, therefore, of the difficulty can be had here.

What makes the matter more perplexing is, that the theorem (26), which is part and parcel of the theorem for the altitudes, accords better with the relation observed between the ratio of the temperatures and that of the densities in the great ascent of Gay-Lussac, when the coefficient of M. Regnault for the expansion is used, than when that of Gay-Lussac is. For example, the upper and lower temperatures of the ascent being 14°·90 Fahr. and 87°·44, if we use 448° as the distance of true zero temperature below the Fahr. zero, we shall have, by (26),

$$\left(\frac{87{\cdot}44 + 448}{14{\cdot}90 + 448}\right)^5 = 2{\cdot}0775\,;$$

and if we put it 11° farther off, we have

$$\left(\frac{87{\cdot}44 + 459}{14{\cdot}90 + 459}\right)^5 = 2{\cdot}0041\,;$$

the latter being much nearer the observed 2 than the former. It is true, either of them is sufficiently close to be within the limits of the probable errors of observation; but M. Regnault's coefficient comes considerably nearer than the old one.

Such, then, is the embarrassing nature of this question, that M. Gay-Lussac's coefficient furnishes altitudes which agree almost exactly with observations, while M. Regnault's gives altitudes apparently too high by perhaps 2·2 per cent. On the other hand, Gay-Lussac's coefficient does not agree nearly so well as M. Regnault's with another marked consequence from the same theorem, in which trifling errors show themselves very conspicuously. It may, perhaps, be remarked that Gay-Lussac's observation of densities and temperatures is a solitary one, and should not weigh against the multitude of experiments on the other side. Granting this, and granting, moreover, that the observed temperatures may be out a trifle, still the character of the observation is such as to claim great confidence in the comparison it affords with a theorem such as ours. For, in the first place, the depression of temperature is very great, and in the next the high power, the fifth of the temperatures, is calculated very much to magnify any errors in the theorem, and likewise to some extent errors in the distance of the true zero.

The more, therefore, these matters are considered abstractedly, the more perplexing they become. Thanks, however, to M. Ramond, we are enabled to solve this knotty problem of conflicting results.

That acute philosopher has observed, that the hour of the day has a very considerable influence on the computed results by the barometric formula. He shows by numerous observations, given in our pages 218 and 219, that about noon the computed results all come out higher than at hours some distance off; and he remarks that those noon computations agree better with the altitudes than the computations of any other hours, on which account he prefers them, and recommends noon as the best time.

By 14 observations at 7 a.m., and 23 at noon, at one place, he finds the latter computed altitudes 7 metres on 251 higher; that is, 2·6 per cent.

In another set of experiments, at a different place, consisting of 12 at 6 a.m. and 38 at noon, the latter are 21 on 915 higher than the former; or 2·3 per cent.

A third set, at a third place, makes a rise of 16 at 12½ hours noon, on 1663 at 10 a.m., or 0·72 per cent.

Another set, at the same place, gave 1652 at 10 a.m., and 18 above it at 1 p.m., or 1·09 per cent.

In a fourth place, the difference was 9 higher at 1 p.m. on 846, computed altitude, at 10 a.m., or 1·07 per cent.

From the concurrent testimony of these experiments, at so many different places, no doubt can exist that the common barometric formula furnishes results considerably higher about noon than at any other time. We do not at present inquire into the cause, but the fact is clear. At all other times except noon, therefore, the theorem, with Gay-Lussac's expansive coefficient, gives results too small. The difference at 6 or 7 hours a.m. and at noon, is just about what we want, that is, above 2 per cent., to reconcile our theorem at 6 or 7 a.m. with observations. Now, as this theorem is computed for the mean state of the atmosphere, and not for any particular hour, it follows, that its results should be those corresponding to the mean times, that is, to times probably about midway between noon and midnight. If M. Regnault's coefficient is used, it is obvious, from what has already been shown, that that would be the case. For, on our present tables, Regnault's coefficient would give numbers 2·2 per cent. higher, which corresponds with what it should be, by the observations about 6 or 7 a.m.

The tables, therefore, in their present state, are calculated to give true altitudes about noon; but at all other times the altitudes will fall short of the truth, and will not show the true mean state of the case.

This partial adaptation may possibly be the reason why Laplace's coefficient in (39) is higher, and his denominator to $C + c$ lower, than our approximate ones in (38).

Having now shown that Regnault's coefficient of expansion, instead of perilling the accuracy of our theorem, as at first it appeared to do, is necessary for its faithful representation of phenomena, in that mean state of the atmosphere which it is its object to represent, it may be expected that we should say something of the anomalous results of the theorems at different hours.

It has been before observed, that this subject is encumbered with great difficulties. M. Ramond looks upon noon to be the time at which the air is more tranquil, and nearer its normal state. That, however, cannot be. The observations concur, about noon, to give the maximum results, and, towards midnight, the minimum. The normal state, therefore, must be between the two. Now it appears to me, that the phenomena are in a great measure, if not wholly, due to the unequal quantities of vapour in the atmosphere, at the different hours. Upward currents, from the heating of the ground at noon, may have some effect; but the large quantities of vapour which are hourly raised into the atmosphere during the day, and redeposited on the earth during the night, must have a very marked effect on the density of the atmosphere.

Thus we have already seen that vapour uniformly distributed, or distributed to very great heights in the atmosphere, cause our general theorem to give lower altitudes than it should. For as it renders the atmosphere specifically lighter, m, the apparent specific gravity of mercury requires to be increased. And so, in our allowance for vapour, we have been obliged to increase the altitudes computed on the supposition of dry, unalloyed atmosphere. Now, if we come to investigate the effects of vapour, supposed to be periodically generated, and diffused into the atmosphere for a few hours, and then withdrawn, a very contrary influence takes place during the time of its generation to that due to a steady intermixture. For as the vapour liberated has the same temperature as the atmosphere, it does not strike any particles of the atmosphere it may come in contact with, with greater force than the particles of the atmosphere do each other; and therefore its intermixture with the lower strata of the atmosphere does not affect the elasticity of strata above, which it has not reached. Travelling slowly to the upper regions, from the interruptions of the atmospheric particles, the continual generation of vapour consequently augments the elasticity of the lower much more than it does the elasticity of the higher strata, and of course raises the barometer higher than it should at the lower station. The consequence is, that the fall of the barometer is more rapid, while this generation of vapour is going on, than its normal rate.

Therefore, while new vapour is being faster generated at the surface of the earth, than it can spread into the higher regions, the computations from the barometer will be higher than they should be. On the other hand, when the ratio of supply to the diffusion begins to diminish, the barometric computations will begin to show less altitudes, and these will go on decreasing as the supply decreases.

This accords with the facts collected by M. Ramond. From the morning to noon, and some time after, as the heat and its concomitant evaporation increase, the computed altitudes rise; and begin to fall as the hours from noon increase, and the temperature and its attendant evaporation diminish.

This is the explanation which presents itself to me of the very singular anomalies in the barometric results noticed by Ramond, and which explanation I submit to philosophers.

If it be the true explanation, the effect of hours will be eliminated, or nearly so, and the computed altitudes rendered consistent, by reducing the altitude of the barometer at each station so much as is due to the excess of the tension of the vapour existing in the atmosphere, above that corresponding to its normal quantity at the density of the atmosphere at the station.

Let philosophers try the rule, and see how far it answers the purpose. If it do, Regnault's coefficient must be introduced into our theorem and tables, and tables of correction for the different quantities of vapour be constructed. If it do not, then we must look further for the cause of the anomaly.

Whether the rule we propose be found true or not, it is pretty evident that none of the logarithmic formulæ, hitherto proposed, show the mean state of the atmosphere, but one of its extreme conditions, namely, that about noon.

BOOK IV.

ON THE EQUILIBRIOUS ACTION OF GASEOUS MEDIA WHEN AGITATED FROM A CENTRE; LAWS OF THEIR ELASTICITIES AND CENTRIPETAL PRESSURES ON BODIES PLACED WITHIN THEM; OR PHENOMENA AND LAWS OF GRAVITATION.

SECT. I.

General Considerations.

Newton, in the early and latter part of his life, propounded a cause for the phenomena of gravitation. He supposed all space to be filled with a highly elastic medium, or ether, which, like any other aeriform body, might be the more rarefied the hotter it is. The nearer to the sun, therefore, the hotter this medium would become, and consequently the more rarefied the ether. Hence, if a body was placed in it he considered that the denser parts would press that body more forcibly than the rarer, and therefore the parts farther from the sun, being more pressed than those nearer, would create a tendency directly towards the sun, "with all that force which we call gravity."

The planets, in the same way, being hotter than the spaces surrounding them, would have an effect on the medium similar to that of the sun, and cause a tendency of other bodies towards them. The same would be the case with all bodies; and hence all the phenomena of attraction which we see in the world may be explained.

So far Newton's idea goes of the cause of gravitation, which, as he says, he has given to show that he does not take gravity to be an essential property of matter. He has left us no notion of how the medium is expanded by heat; nor, if it is so dense as phenomena require it should be, to pervade the pores of all bodies and act on each particle separately, has he shown how bodies can move through such a medium and experience no sensible resistance to their rapid motions. Neither has he explained how the parts of the medium can keep each other in equilibrio, and yet press unequally on the farther and nearer sides of any body within it. These are difficulties in the hypothesis he bequeathed to posterity.

From our views of the nature of airs the phenomena of expansion by heat are obvious. It is also easy to understand how great elasticity, great density, and exceeding activity may be combined and still produce no sensible retardation to bodies in rapid motion. For we have only to suppose the atoms of the medium to be excessively small for them to have exceedingly great velocity, and of course the medium to have a very high degree of activity, and be competent to afford but a very small resistance, though the numeratom of the medium be very great. A medium, therefore, with all the properties Newton required is perfectly reasonable, and it only remains for us to investigate what is the law of the increase of its density from an agitating centre; whether it can keep its parts in equilibrio, and still have an elasticity increasing with the density of the medium; and whether it gives results according with phenomena.

In order to carry out this idea, it is necessary that the atoms of the medium should be kept from flying off by some body or bodies at a great distance, constantly beating them back. It is not necessary that these bodies should all be at the same distance from the sun, nor that they should all beat back the atoms of the medium with equal force. For, if they are at very great distances, any irregularities in their actions would blend one into the other long before they reached the known limits of our planetary system.

Should it be asked what those bodies are that can perform the office of reflectors, we answer the fixed stars, and other parts of the system. To each star, planet, and particle of matter every other star, planet, and particle of matter may act as a reflector, and thus present us with a most magnificent idea of mutually dependent mechanism, and of the wisdom and sublime simplicity of the Great Architect of all. The planets which we see retained in their orbits about the sun, by what we have supposed to be the sun's attraction, are as much indebted for that attraction to the stars beyond whose influence they have been imagined to be, as they are to the sun itself. Nay, if we are able, as we trust we shall be, to demonstrate the law of gravitation, there is not the most trivial natural phenomenon on our globe, depending on attraction—and we know not one that is exempted—which does not in part owe its existence to the smallest star that twinkles in the heavens, however remote it may be, and to the most insignificant insect that crawls upon the earth. There is a mutual action, reaction, and dependence of all things throughout the universe one upon the other; not one is in a state of independent existence.

In this theory of gravitation, the law of attraction will be very different at distances to which the diameters of the particles are relatively sensible, to the law at distances to which the said diameters are relatively insensible. At the former the force will be much stronger than the general law would give, and it will follow a law increasing more rapidly with a diminished distance than the inverse duplicate of the distances. That law will go on growing up to the point of contact, at which the force of attraction will be vastly greater than at any the smallest conceivable distance.

Newton has suggested a number of other phenomena as the consequence of his ether. He supposes it may be the cause of refraction and reflection, and that by the rapidity of its vibrations it may overtake and impart to light certain properties which it is known to have. For example, he thinks the ethereal waves or pulses overtaking light may put it in a state of alternate ease and difficulty to be reflected and transmitted, and thus be the cause of the phenomena known under the name of "the fits of easy transmission and reflection." He also supposes it may be the agent by which the will is transmitted from the brain to the muscles; and generally that it is the agent of volition.

With regard to the cause of refraction, reflection, and the phenomena of the fits of easy transmission and reflection of light, we think we shall be able to explain that hereafter on much simpler principles; but those things connected with the propagation of the power of the will to the muscles, not being the subjects of calculation, do not come under the class of phenomena to be discussed in this work.

Definition.

The force which an air exerts on the surface of any body within it or containing it is called its elasticity; but if two portions of gas maintain each other in a state of quiescence by their mutual actions, the force which each exerts on the other I call its *equilibrious action.* This action is different from that of elasticity, and follows a different law; it depends entirely on the relative numeratoms and atomic velocities, whilst the elasticity depends on the numeratom and atomic momenta.

PROPOSITION I.

If the numeratoms of two gaseous strata, keeping each other in equilibrio, preserve an invariable ratio, the atomic velocities will also preserve an invariable ratio; and conversely if the ratio of the velocities be invariable, so will also be that of the numeratoms.

We do not here intend to inquire in what manner the atoms move among one another, or how they strike to keep up the equilibrium. These are things to which we can perhaps never assign laws, and therefore we must endeavour to make our investigations independent of them.

Let us first suppose the numeratoms to remain invariable. Then if the velocities of the atoms of either stratum change, the intensity of their action and the periods of their revolution will manifestly change. In order, therefore, to preserve the balance of action between the two strata, the force and periods of the other atoms must receive a proportional change. But since the numeratoms are unchanged, these things can be effected by a proportional increase or diminution only of velocity; in which case the periods and intensities of action will respectively preserve their ratios unaltered, and the equilibrium will be maintained. And if we suppose the numeratoms to change, and yet to keep always the same ratio, the equilibrium will still be continued. For as the increase or decrease of the numeratom only contracts or dilates the path of the atoms, without affecting the figure of those paths, the atoms will move similarly in the dilated and contracted paths; and if the numeratoms and velocities have the same ratios, the manner of collision and the ratio of the periods will be the same, and consequently the equilibrium will remain as before.

Cor. If the ratio of the numeratoms be invariable, the difference between the velocities of the atoms in the two strata will be as the velocities of the atoms in either stratum.

PROPOSITION II.

In two strata keeping each other in equilibrio, the duplicate ratio of the atomic velocities is equal to the inverse subtriplicate ratio of the numeratoms.

First. Setting aside all idea of the mutual actions of the particles of the same stratum, let us suppose the particles of the two strata to keep each other in equilibrio by moving in right lines from and towards one another. Then if the particles in each stratum be uniformly disposed throughout, it is plain that to keep up a constant equilibrium there must be the same number of particles in each stratum; for if not, the particles of one stratum would not be reflected by those of the other, but would, at least many of them, pass by and produce a confused intermixture. But if there be the same number of particles in each stratum, and they move in this manner, the equilibrious action of each particle, for a given length of time, must be the same in each stratum. Therefore if p represent the number of periods of vibration in a given time of one of the particles in one stratum, v the velocity of the particle, and n the numeratom of the medium; and if P, V, N represent the like things of a particle in the other stratum, $pv = PV$. But p is as $vn^{\frac{1}{3}}$, and therefore

$$v^2 n^{\frac{1}{3}} = V^2 N^{\frac{1}{3}},$$

the gas being supposed homogeneous.

Secondly. If the particles, instead of moving directly to and from one another, have any other motion, yet as it is by the direct part of their motion only that the strata preserve the equilibrium, it follows, for the reasons we have given above, that there must be the same number of particles in each stratum, and that the vertical equilibrious action of any two opposing particles must be the same. But the period of a particle and the time of its vertical vibration are the same, and its vertical and periodic velocities are proportional; therefore, if the two opposing particles in the two strata describe similar paths, $v^2n^{\frac{1}{3}} = V^2N^{\frac{1}{3}}$. Or if they do not describe similar paths, yet, since the vertical and periodic velocities of a particle have an invariable ratio,

$$v^2n^{\frac{1}{3}} \propto V^2N^{\frac{1}{3}} \quad . \quad . \quad (1).$$

Cor. From this theorem it follows that in any gaseous medium, whose particles have no particular tendency or are not acted on by any soliciting force, the equilibrious action of each particle estimated in the same direction is the same.

Scholium.

Should there be a difficulty in conceiving how the particles of the two strata can act on one another, so that all the particles of one stratum may have the same periods, we might imagine the two strata to be again equally divided into strata parallel to the contiguous surfaces, and that the inmost stratum of one is kept in equilibrio by the outmost stratum of the other, and the outmost but one by the inmost but one, and so on.

Lemma.

In cases where a cause produces an effect, through any modifying—but not augmenting or diminishing—circumstances, the differential of the effect is equal to the cause reduced to the individual case and the effect divided by the normal effect conjointly, drawn into the differential of the time. That is, if f be the cause or force, e the normal effect that would take place without any modifying circumstances, e the effect that does take place under the circumstances, and t the time,

$$de = \pm \frac{e}{\mathrm{e}} fdt \qquad . \qquad . \qquad (2).$$

For if the modifying circumstances be nothing or constantly the same, the variation of effect at any point of time will be evidently as the generating force or cause. And if the force or cause be the same, this variation—the effect being less as the modifying circumstances are greater, and greater as these circumstances are less—will be as the effect itself. Therefore, neither being the same, the momentary variation of effect will be as the force and effect conjointly; and the differential of the effect will be equal to the force, effect, and differential of the time divided by the normal effect.

Cor. If the modifying circumstances be nothing, that is, if the effect be normal, this theorem coincides with the common theorem $de = \pm fdt$.

PROPOSITION III.

If an agitating particle be put in an indefinite medium, whose atoms have no attractive tendency, it is required to find the law of the variation of the atomic velocity of the medium, and of the numeratom and elasticity; supposing the atoms to derive their motions exclusively from the central agitation, and that at the limits of the medium the atoms are beaten back with a force just sufficient to counteract that arising from the agitation at the centre.

Since we suppose the atoms to be absolutely hard, and the agitations to be propagated in right lines every way about, the intensity of the agitation, as felt on a given space at the distance x, must be as

$$\frac{1}{x^2};$$

and on a given particle as

$$\frac{1}{x^2 n}.$$

But if $-\dot{v}$ be the decrement of the atomic velocity, this decrement will be as the force $\frac{1}{x^2 n}$ which occasions it, and as the velocity itself conjointly; for in order to preserve the equilibrium, the numeratom remaining the same, the decrement or difference of atomic velocity between two contiguous strata of the medium must, *Cor.* Proposition 1, vary as the velocity. Therefore

$$-\dot{v} \propto \frac{v}{x^2 n};$$

which is a decrement corresponding to a distance $\frac{1}{n^{\frac{1}{3}}}$, the extent of range of one of the particles, giving

$$\frac{1}{n^{\frac{1}{3}}} : \dot{v} :: dx : dv = \dot{v}n^{\frac{1}{3}}dx.$$

Consequently,

$$-dv = \frac{Adx.v}{x^2 n^{\frac{2}{3}}} = \frac{Av^5dx}{x^2}$$

by (1), A being as the force of central agitation. The integral of this being taken gives

$$\frac{1}{4}\left(\frac{1}{v^4} - \frac{1}{V^4}\right) = A\left(\frac{1}{X} - \frac{1}{x}\right)$$

$$\text{or} \qquad v = \frac{V\sqrt[4]{Xx}}{\sqrt[4]{\{Xx + 4AV^4(x - X)\}}} \qquad (3)$$

in which V is the velocity at some fixed distance X.

Again, because $n \propto \frac{1}{v^6}$,

$$n \propto \left\{\frac{Xx + 4AV^4(x - X)}{V^4Xx}\right\}^{\frac{3}{2}}, \quad . \quad . \quad . \quad (4)$$

and e being the elasticity at x

$$e \propto nv^2 \propto \frac{xX + 4AV^4(x - X)}{V^4Xx};$$

$$\text{or} \qquad e \propto \frac{1}{V^4} + 4A\left(\frac{1}{X} - \frac{1}{x}\right). \qquad (5).$$

Otherwise thus: By the preceding Proposition

$n^{\frac{1}{3}}v^2$ is a constant quantity when there is no tendency in the atoms of the medium to or from any centre of force. Therefore

$$-\dot{v} = \frac{v\dot{n}}{6n},$$

in which $\dot{v}$ and $\dot{n}$ are the incremental differences between contiguous strata. But the atoms being perfectly hard the agitating force is communicated from stratum to stratum with undiminished intensity, except so far as it is diminished by being diffused over a greater space. The effect, therefore, is the same as if the agitating force acted immediately on the particular stratum, and consequently $\dot{n}$, which is the result of that action, is proportional to it. Hence, A being the force of agitation at the distance X

$$-\dot{v} = \frac{AX^2v}{6nx^2}$$

This is the value of $\dot{v}$ corresponding to a distance $\frac{1}{n^{\frac{1}{3}}}$ or $\dot{x}$ the increment of x. Therefore, since $\frac{1}{n^{\frac{1}{3}}} = \dot{x}$

$$-\dot{v} = \frac{AX^2v\dot{x}}{6n^{\frac{2}{3}}x^2} \text{ or } -dv = \frac{AX^2v}{6n^{\frac{2}{3}}x^2}dx$$

which will give

$$\frac{1}{v^4} - \frac{1}{V^4} = \frac{2A}{3}\left(X - \frac{X^2}{x}\right) \quad . \quad . \quad . \quad . \quad (6)$$

for its integral, the same in substance as (3).

Cor. In a medium, therefore, whose atoms are not solicited by any force, the elasticity

$$e \propto \frac{1}{v^4} \propto \frac{1}{n^{\frac{2}{3}}} \text{ or } e = A - \frac{B}{x} \quad . \quad . \quad . \quad (7),$$

where A and B are constant quantities.

PROPOSITION IV. PROBLEM.

A body being placed in such a medium as above, it is required to find the law of the force with which it is impelled towards the agitating centre.

Let $AHFG$, *fig.* 1 be the plane of any section of a body in which Cx, Cy are two rectangular axes; and let from any point B in the surface of the body, the perpendicular BK, fall on this plane. From a point D, infinitely near to B, let fall another perpendicular DL. Draw KL meeting BI perpendicular to BD in I; and let BE be parallel to KL. Then the triangles DBE, IBK are similar, and if $e \times BD$ be the elastic force of the medium on the line BD, that is in the direction BI, we shall have force on BD in direction BI, to force on BD in direction $BK : : BI : BK : : BD$ to BE or KL; that is, if q denote the pressure on BD in a direction perpendicular to the plane yCx

$$e \times BD : q : : BD : BE, \text{ or } q = e \times BE.$$

Hence the pressure on the differential rectangle $dydx$ is also $edydx$; and the total pressure on the body in a direction parallel to BK or to the axis z, is

$$\int^2 edydx \quad . \quad . \quad . \quad . \quad . \quad . \quad (8).$$

In the same way it is shown that the pressures on the whole body, in the directions parallel to the other axes x, y, are respectively

$$\int^2 e\,dy\,dz \text{ and } \int^2 e\,dx\,dz \;.\quad .\quad .\quad (9).$$

Now by (7) e is given in a function of the central distance, which might easily be transformed into a function of one or more of the axes, because the distance of the body is supposed to remain invariable. Hence, therefore, the pressures in the directions of each of the axes might be determined, and consequently the direction in which the body is urged, and the force with which it is urged, might be found.

We shall now give a few examples of the laws of this pressure.

Example 1. Suppose the body be a sphere, the distance of whose centre from that of agitation let be p, and of any point in its superficies be v; and suppose the axis z be parallel to p. Then the plane yx being that on which the pressure urges the body directly towards the centre of agitation, $\int^2 \left(A - \frac{B}{v}\right) dy\,dx$ will be the total tendency, where $v = \sqrt{p^2 + r^2 + 2pz}$, and r is the radius of the sphere.

Now if we take z constant, $y^2 + x^2$ is constant, and $\int dy\,dx = 2\pi t\,dt$, where $t^2 = y^2 + x^2$; for $dy\,dx$ is equal to the differential of the perimeter of a circle, whose radius is t drawn into dt. Therefore, and because $r^2 = z^2 + t^2$,

$$\int^2 \left(A - \frac{B}{v}\right) dydx = \int \left(A - \frac{B}{v}\right) 2\pi t dt =$$

$$\int \left(A - \frac{B}{\sqrt{p^2 + r^2 + 2pz}}\right) \times -2\pi z dz = \qquad (10).$$

$$-\frac{2A\pi z^2}{2} + 2B\pi \left\{ \frac{z}{p} \sqrt{p^2 + r^2 + 2pz} - \frac{(p^2 + r^2 + 2pz)^{\frac{3}{2}}}{3p^2} \right\}$$

In order to correct this integral we must observe that when $x = + r$ or $- r$ the integral must be null. Therefore if from its value when $z = 0$ we take its value when $z = + r$, the difference will be the pressure on the upper hemisphere towards the centre of agitation. And if in like manner we take its value when $z = - r$ from that when $z = 0$, we shall have the pressure on the under hemisphere in an opposite direction. The latter, therefore, taken from the former will leave the total pressure towards the centre of agitation, which, after due reduction, is found to be $\frac{2B\pi r^3}{3p^2}$. That is, the whole tendency is as the force of agitation and the magnitude of the sphere directly, and as the square of its distance inversely from the centre of agitation.

Example 2. Suppose the body be an ellipsoid, whose major and minor semiaxes are a and b, and the major in a line with the distance p joining the centres. Then (10) in the preceding example will be the same, and do for this except the value of v. Using the same notation, we have $a^2 : b^2 :: a^2 - z^2 : t^2$, and therefore $t^2 = \frac{b^2}{a^2}(a^2 - z^2)$ and $a^2 t dt = - b^2 z dz$. Whence, since $v^2 = (p + z)^2 + t^2 = p^2 + 2pz + z^2 +$

$b^2 - \frac{b^2}{a^2} z^2 = p^2 + 2pz + b^2 + \frac{a^2 - b^2}{a^2} z^2$, our (10) becomes

$$\int \left\{ -\frac{2A\pi b^2 z dz}{a^2} + \frac{2B\pi b^2 z dz}{a^2 v} \right\},$$

which, as in the preceding example, must vanish when $z = +a$, or $-a$. It is also evident that the first term of this integral disappears in the difference of pressures on the two same ellipsoids, expressing the total tendency towards the centre of agitation. Our concern is, therefore, only with the integral of $\frac{2B\pi b^2 z dz}{a^2 v}$; and, indeed, only with

$$\frac{2B\pi b^2}{a^2} \left\{ \int_{z = \quad a} \frac{zdz}{v} - \int_{x = a} \frac{zdz}{v} \right\} \quad . \quad . \quad . \quad (11),$$

which is the value of the total tendency towards the centre of agitation. But

$$\int \frac{zdz}{v} = \frac{a^2 v}{a^2 - b^2} - \frac{pa^3}{(a^2 - b^2)^{\frac{3}{2}}} \log \left\{ 2 \frac{a^2 - b^2}{a^2} z + 2p + 2v \sqrt{\frac{a^2 - b^2}{a^2}} \right\}$$

which developed, and duly reduced, first putting $\sqrt{a^2 - b^2} = e$, gives for (11) the whole tendency

$$4B\pi b^2 a \,.\, \left\{ \frac{1}{3p^2} + \frac{e^2}{5p^4} + \frac{e^4}{7p^6} + \,.\,. \right\} \quad . \quad . \quad . \quad (12);$$

that is, since at all sensible distances e the eccentricity must be infinitely small compared with p, it is very nearly

$$\frac{4B\pi b^2 a}{3p^2};$$

or as the central agitation B directly, the volume $\pi b^2 a$ directly, and the square p^2 of the distances inversely.

If instead of the axis major coinciding with the direction of p, the axis minor does, the result will be the same, except that a and b mutually change places. In this case every odd power of e^2 is negative and every even power positive, so that (11) becomes

$$4B\pi a^2 b\left\{\frac{1}{3p^2}-\frac{e^2}{5p^4}+\frac{e^4}{7p^6}-\frac{e^6}{9p^8}+\ldots\right\} \quad . \ . \ (13).$$

Hence the same law evidently holds good; but the total force is different, it being greater in the latter case than in the former; that is, when the major axis is perpendicular to p, the impressing force is greater than when it is in a line with p.

This property we shall have hereafter to apply to most important purposes.

Example 3. Let the body be a rectangular parallelopiped, whose sides are $2a$, $2b$, $2c$, and suppose the distance of its centre of gravity from the centre of agitation be p, and let $2a$ be parallel to p. This will make the plane of b, c perpendicular to p. Then our

$$\int^2\left(A-\frac{B}{v}\right)dydz=$$

$$\int^2\left(A-\frac{B}{\sqrt{(p+a)^2+y^2+z^2}}\right)dydz=$$

$$\int\left\{Aydz-Bdz\log\left(y+\sqrt{(p+a)^2+y^2+z^2}\right)\right\}$$

which taken from $y = -b$ to $y = +b$ becomes

$$\int\left\{2Abdz + Bdz\log\left(\frac{\sqrt{(p+a)^2+b^2+z^2}-b}{\sqrt{(p+a)^2+b^2+z^2}+b}\right)\right\} \quad (14).$$

Now, as in the preceding case, the part affected with A disappears, when, after the next integration, we take the pressure on the lower surface from that on the upper. We shall, therefore not encumber ourselves with it. Then, putting q^2 for $(p+a)^2$, we shall have for the integral sought, unaffected by B,

$$z\log\frac{\sqrt{q^2+b^2+z^2}-b}{\sqrt{q^2+b^2+z^2}+b} - b\log\left\{2z^2+b^2+q^2+2z\sqrt{z^2+b^2+q^2}\right\}$$
$$+a\sqrt{-1}\,.\log\frac{b^2z^2+(q^2-b^2-z^2)q^2+2bqz\sqrt{-z^2-b^2-q^2}}{z^2+q^2} \quad (15)$$

To correct this integral we must take it from $z = -c$ to $z = +c$, which would give the pressure on the upper surface; and by changing afterwards $q = p + a$ into $q = p - a$, it would give the pressure on the under surface. The latter taken from the former leaves the total tendency towards the centre of agitation. But as the management of this ultimate expression would be troublesome from its complexity, we may save a great deal by considering that, at all sensible distances, the dimensions a, b, c of the body are insignificant in comparison with p, the distance from the centre of agitation. Hence (15) corrected becomes

$$2c\log\frac{1-\frac{b}{q}}{1+\frac{b}{q}}+b\log\frac{1-\frac{2c}{q}}{1+\frac{2c}{q}}+a\sqrt{-1}\,.\log\frac{1+\frac{2bc}{q^2}\sqrt{-1}}{1-\frac{2bc}{q^2}\sqrt{-1}}\ ..(16).$$

And if q be changed into $p+a$, and then into $p-a$, and the expression with the latter be taken from that with the former, we obtain

$$8abc\left\{\frac{1}{p^2}+\frac{b^2+2^2c^2}{3p^4}+\frac{b^4+2^4c^4}{5p^6}+\,.\,.\right\}$$

$$+\,16a^2bc\left\{\frac{1}{p^3}-\frac{2^2b^2c^2}{3p^7}+\frac{2^4b^4c^4}{5p^{11}}-\,.\,.\right\},$$

which, because b, c are extremely small compared with p, sinks eventually to $\frac{8abc}{p^2}$, or the body directly and the square of the distance inversely.

We see from the above expression that the quantities b, c are not equally involved, which, as they are both in the same plane it is obvious they ought to be. This arises from (16) being itself merely approximative. If we are desirous of obtaining an accurate value in series, it may be obtained at once from the primitive expression. For putting, as before, $q=p+a$,

$$-\int^2\frac{dydz}{\sqrt{(p+a)^2+y^2+z^2}}=$$

$$-\int^2 dydz\left\{\frac{1}{q}-\frac{y^2+z^2}{2q^3}+\frac{3(y^2+z^2)^2}{2^3q^5}-\frac{3\,.\,5(y^2+z^2)^3}{2^4\,.\,3\,.\,q^7}+\,.\,.\right\},$$

an expression which, neglecting all the terms excep the first, and then subducting the value with $q=p-a$ from the value with $q=p+a$, gives

$$\frac{8abcB}{p^2-a^2}=\frac{8abcB}{p^2} \quad . \quad . \quad . \quad (17)$$

the same as before

Hence the following law, namely:

That the centripetal tendency of all bodies, at such distances that the dimensions of the bodies are insignificant, compared with their distance from the centre of agitation, is as the volume of the body directly, the force of central agitation directly, and the square of the distance of the centre of the body from the centre of agitation inversely.

Cor. When the dimensions of the body bear a significant ratio to the central distances, the law alters materially, and becomes mixed up with the figure and the position of the body.

PROPOSITION V. *Theorem.*

If a medium, such as before described, be agitated by the vibrations of a small body at the centre, the force of agitation felt at a distance from the said centre, is as the intensity of the central agitation, and the volume of the agitating body, both directly.

For the accumulated effect of agitation at any distant point, must evidently be as the intensity upon any one atom at the centre, and the number affected or disturbed by the agitation. Now, by our principles of collision for hard bodies, the reflected intensity upon any separate atom, whether the atom receive a part or the whole of the stroke, would be as the momentum of the agitating body; that is, as its temperature. But the number agitated would be as the number disturbed or removed by the motions of the agitating body, that is, as the volume of that body. Therefore, the intensity of agitation, as felt at a distance from the centre, is as the product of the volume of the body and its temperature.

Cor. 1. Hence we have the following laws of centripetal tendency at all considerable distances:

LAWS.

1st. *Other things alike, the force of a centripetal tendency is as the volume of the tending particle.*

2d. *The volume being the same, and other things alike, the centripetal tendency or force is as the square of the distance inversely of the centre of the particle from the centre of agitation.*

3d. *Things being the same with regard to other matters, the centripetal force is as the central force of agitation, or as the temperature.*

4th. *The intensity of the centripetal force is as the volume of the agitating small body.*

Cor. 2. Consequently, the complete law of centripetal tendency is made up of all the above laws conjointly.

Cor. 3. If we suppose matter to be uniformly dense, the masses may be substituted for the volumes in either case.

Scholium.

The laws which are developed in *Cor.* 1 are those which have been shown by astronomers to exist in the planetary system, with the exception of the 3d, which is yet to be recognised. Nothing, as will presently be shown, has been discovered adverse to this 3d law, while there are certain phenomena in its favour.

A medium, therefore, such as Newton supposed, will give the same law of attraction with respect to distance, which is observed to govern the motions of the heavenly bodies, and which, by the experiments of Cavendish, Baily, Reich, &c. is found to exist in all bodies.

If matter be of uniform density, it will give the law which is recognised with regard to mass, as observed in *Cor.* 3, for the volume will then be proportional to the mass. Now, we have no evidence which I know of that all matter is of equal or the same density, nor that it is not. We cannot, therefore, prove or disprove this part of our law; but phenomena render it probable that the ultimate atoms of matter—and it is of those only we speak—are all of the same density; for where no pores exist we cannot well conceive how inequality of density can be. It is easy to understand how the Almighty may have fashioned into various sizes and figures his elements; but there is a difficulty in conceiving how, without pores or parts, one element should be more massive than another, and of the same volume. However, it is not impossible.

The third condition of our Law is, that the intensity of the centripetal tendency is directly as the force of central agitation. This is a point, as before observed, that remains to be proved. If we regard bodies, this force of agitation corresponds with their temperature, and hence we have this law, namely, that the force of the attraction is directly as the temperature of the body. Therefore the intensity of attraction in any body, at the temperature of water-freezing, is to the intensity with which it attracts at the temperature of water-boiling, as 29 to 34, or approximately as 6 to 7, for such is the ratio of the true temperatures at those points.

The proof of this influence of heat on attraction is very desirable. Nothing, however, direct is known to favour it, but there are some indirect things. For example, it is mentioned in Rees's Cyclopædia that Euler found the focal distance of a long object glass contracted on being heated. If, therefore, the refraction be due to attractive forces in the glass, it shows that the intensity of that attraction is stronger the hotter the glass is; and if it is so in one case we may reasonably suppose it to be so in others.

We have also another correlative example. It is found that the densities, as it is called, of the planets, decrease as they recede from the sun, and nearly as their distances increase. In other words, that equal volumes of the planetary masses do attract more forcibly the nearer the planets are to the sun. This has been attributed to the planets themselves being less and less dense as they recede from the sun. We shall, however, be able to show that their temperatures follow the very law which phenomena point out as being that of their attractions. Without, therefore, any hypothesis of a different density, we can explain those phenomena, and consequently bring an indirect evidence to support that part of our law which has not hitherto been able to be settled by direct experiment.

I was in hopes that the lunar theory might lend some assistance. For as the earth is nearer the sun in the winter than it is in the summer, and therefore more strongly heated in the former than in the latter season, I thought it might possibly produce some effect on the mean annual equation of the moon. The values of this equation, as determined from theory by M. Damoiseau and M. Plana, differ however about 5″; so that nothing can be concluded from their computations of this equation and those equations deduced from observations. Indeed the eccentricity of the earth's orbit is so small, and the time the earth is in the inferior part of it is so short, that no excess of heat which the earth receives during her passage through that inferior part could sensibly affect her average temperature.

A most extensive series of experiments was undertaken, a few years since, by the late Mr. Francis Baily, at the desire of the British Association, for the purpose of determining the mean density of the earth, and the effect of temperature on gravitation. I had been myself previously engaged in some similar experiments, not with the view of determining the density of the earth, but whether bodies of different kinds did attract one and the same body in proportion to the attracting masses. These experiments produced some curious results, but as they were interrupted and broken by my removal from the house in which I lived, and as I have had no opportunity since of repeating them, and trying others I had projected, they have been thrown by and neglected: they were, indeed, preliminary experiments, intended as pioneers to a more accurate course. By these experiments, which I continued through some months, I acquired experience in the difficulties of the operation, and offered to lend the apparatus I had, which was very delicate, to Mr. Baily, and I tendered him any assistance or suggestions I had acquired by my experience; but I suppose he thought he knew better about these matters than I did, and therefore I was thanked and my offers not accepted. He, however, afterwards politely sent me a copy of his experiments, and I must do him the justice to say, that they appear to have been well and carefully made. He has recorded above 2150 experiments, besides those he rejected. It is a most extensive series, and one experiment with the other accords pretty well as to determining the mean density of the earth, though the result does not agree with Cavendish's or Reich's.

In the grand object of determining the effect of heat on gravitation, they fail. Mr. Baily has carried on his experiments from a temperature of 31°·383 to a temperature of 68°·443. With this difference of temperature the attraction ought to be 3·8 per cent. greater in one case than in the other; but Mr. Baily's experiments—no doubt from the great difficulty of making them—differ in the mean of the same series at nearly the same temperature (30° and 30°·575) from a mean density of the earth of 5·2886 to 5·8715, or about 11 per cent.

One cause, I conceive, of the anomalies in Mr. Baily's experiments was the locality in which they were carried on. It was an upper room in his house in Tavistock place, that is, nearly in the heart of London. From the rattling of the carriages, changing of fires, and operations of all sorts constantly going on in a great town like London, I certainly think his house was not a desirable place for such very delicate experiments. At Cranford, 12 miles out of London, where I tried mine, I did not meet with the irregularities Mr. Baily describes, though I encountered others which he has not mentioned.

But there is one circumstance which would interfere with the effect of the change of temperature in Mr. Baily's experiments, and upset the result. For example, the earth and places in the neighbourhood of his apparatus were not in the same constant state, under these great changes of temperature. The temperature of all these places, which, being so near, would have a great influence in modifying the earth's apparent attraction, was the same as the temperature of Mr. Baily's attracting masses, and therefore made the attraction of the earth appear to rise and fall with the attraction of the masses. If Mr. Baily had allowed me, it was my intention to suggest to him the necessity of having a place so built that the masses and all the apparatus together might be heated up to 212°, and maintained at that temperature in an atmosphere of the same 212° temperature, during any state of the external temperature. If a house with this object was built, it should be circular, with an internal casing some distance from the external wall, like an ice-house. The outside should also, as far as possible, be protected from the rays of the sun.

The light illuminating the arc of vibration should be diffused daylight, for, in the apparatus I used, I found the light of an oil lamp, concentrated by a large lens, had a very sensible effect in disturbing the experiments; so much so, that I was obliged to desist from making experiments at night.

Mr. Cavendish employed spherical masses of lead, each weighing 2,439,000 grains, or, at 7000 grains to the lb., 348·43 lbs.; Mr. Baily's were also spheres of lead, each weighing 380·469 lbs.: and M. Reich, I gather from Mr. Baily's account of these experiments (p. 48), employed masses of iron, but of what weight I do not know, never having seen the original article. The masses I used were spheres of 4 lbs. and 9 lbs. of lead; 13 inch iron shells, 193 lbs. each, and the largest iron shot. The shells and shot were lent me by Mr. Charles Hood, F.R.S., of the firm of Thomas and Charles Hood, Earl street, Blackfriars. They had been very carefully selected from others in their yard, and weighed by Mr. C. Hood. I also had constructed two copper spheres, about 12 inches diameter, as thin as they could be consistently with strength. These I filled with water, wheat, &c. They are the only part of my apparatus now left.

The mean densities of the earth, as given by these experiments, Cavendish's being corrected of an error in the calculation by Mr. Baily, are

Cavendish . . .	5·448	with lead
Reich	5·4439	iron ?
„	5·4383	do. ?
„	5·4522	iron
Baily	5·6747	lead

My own experiments, as before observed, had no object of this kind, and are now lying unreduced somewhere I believe among my papers.

It appears from the above abstract that Mr. Baily's experiments are about 4 per cent. more than the other two, which agree remarkably well with each other. The difference between the results of Mr. Baily and either of the others shows that none yet made are sufficient to determine the effect of temperature, and that new experiments are wanting at wider ranges of temperature and under more favorable circumstances.

When I once spoke to Mr. Baily on the subject of the effects of temperature—some short time, I think, before his lamented accident—he informed me that he had noticed a difference, but the observations were so irregular he could make nothing of them. It is much, therefore, to be wished that a new set of experiments were instituted, for the especial purpose of settling the question of temperature. It is a point well worthy the labours of those who have a place fit to make the experiments in. The reward would be the determining of a very important question mooted by our illustrious Newton, namely, whether gravity be or be not an essential property of matter. If heat has any influence on the intensity of attraction, it is evident that that force which we call gravitation is not a property of matter. The experiment before alluded to by Euler with a heated lens is not, perhaps, quite conclusive, as it may be considered to respect another kind of force. And if it was unexceptionable, it does not determine the quantity of the influence.

Mr. Baily's experiments appear to have been made, like Mr. Cavendish's, with two masses of lead only. Neither seems to have thought of trying the effect with masses of different bodies. It may have been with Mr. Cavendish the difficulty of procuring masses of different materials large enough; but that does not appear to have been the case with Mr. Baily. He evidently has relied upon the principle of action and reaction holding good in attraction as it does in corporal collisions; for he has employed for the small balls lead, brass, zinc, platina, ivory, and glass. Upon all these the leaden masses exerted an equal force upon equal weights, and therefore he seems to have concluded that had these several substances been the masses, and the balls lead or anything else, the attraction would have exhibited the same specific intensity. By our 5th Proposition that, indeed, appears to be the case, but in experiments of this nature it strikes me that the thing should have been directly verified.

Happily M. Reich's experiments in part supply the hiatus. This philosopher, in one at least of his sets of experiments, used masses of iron. Now the capacity or numeratom of iron is ·11, and that of lead is ·0293 or $3\frac{3}{4}$ times less. Therefore, if the number of particles and not the mass had determined the intensity of attraction, the iron would have attracted $3\frac{3}{4}$ times more powerfully than the lead, and the mean density of the earth would have come out $3\frac{3}{4}$ times less in M. Reich's experiments than it does in Cavendish or Baily's. But the results are very nearly the same in all, which accords with our 5th Proposition as being one of the consequences of Newton's medium, or as he calls it ether.

An objection may be raised to this medium on the score of its want of activity to represent phenomena. Laplace says, in the 15th chapter of the 4th book of his 'System of the World,' that he had found it "certain that the activity of gravitation is much greater than" six million times the velocity of light. Admitting this, it forms no objection at all to the ether; for the resistance will depend upon the activity of the ether, and the activity of the ether will depend on the smallness of its atoms. Suppose one of the ethereal atoms to have the same ratio to a particle of light, which a ball a foot diameter has to the whole earth (and there is nothing which forbids us to suppose that the ratio may not be as little or even much less), then calling the earth 8000 miles or 42,000,000 feet diameter, a thousand of these ethereal atoms impelled together by the same force which gives motion to a particle of light, would each receive a velocity more than 74,000,000,000,000,000,000 times greater than that of light. But with such an activity, the law of attraction on a body moving with the velocity of light in the direction of the attraction, would not be augmented more than the $\frac{54}{1,000,000,000,000,000,000,000}$ part of unity. With this increase in the law of attraction, it would require 2,857,796,067,672,610 years before the apses of the earth's orbit would move one second of a degree. But since the calculation is made on the supposition that the body withdraws itself, as it were, from the action of the impelling power with the rapidity of light, the augmentation or diminution of the law, on a body moving like any of the planets of our system, would be many million times less; and therefore it would require many million times the period above assigned to produce, with a fluid of such activity, a difference of 1″ in the position of the earth's apogee or perigee. We may hence fairly conclude, that there might be an ether pervading the heavens and all bodies, of such activity, that no sensible difference could be observed in the intensity of its action on bodies in a state of quiescence, or moving with a velocity not only six million, but several million million times greater than that of light.

In a somewhat similar way may we show that the resistance of the ether could not, in an immensely long period of years, produce any appreciable resistance to bodies moving much faster through it than light, though the ether's numeratom be exceedingly great.

A third objection may be started on the view that ether could not pervade the pores of a body and act on each particle separately so as to secure that independence of attraction on other bodies at a distance, which we know exists in the particles of bodies. Now, Newton, in his Optics, has shown bodies to be so porous that water has at least 40 times more pores than solid parts; and M. Biot, in his 'Traité de Physique,' t. iv, pp. 124 and 125, has boldly broached the idea that the solar system itself may be but one great particle relatively to other larger and rarer systems. He thinks it probable that in the densest bodies the volume of the pores will exceed by many thousand times the volume of the solid parts. Admitting such a state of things, there will be no difficulty in the ether surrounding and acting on every particle separately, and being acted on again separately by it. Besides, by our theory of heat, the particles are not quiescently touching each other. They are in a constant state of agitation, striking one another and flying off; so that in their mean state they really do not touch but keep at a distance, and may be separately surrounded. Objections therefore against this ether on account of the congregation of the particles or defect of pores, would fail.

What is said of the tendency or attraction, has been on the supposition of a single agitating particle. The results are, however, evidently the same if the agitation comes from several particles grouped together after the manner of any known body. For each particle would affect the ether separately in its own way, and produce a tendency or attraction, on any other atom, peculiar to itself and according to its own intensity of agitation and volume. Therefore the congregation of agitating particles would produce an effect on any distant particle, equal to the sum of all the separate effects due to each if each acted singly and alone.

Regarding the centripetal tendencies, consequently, as so many attractions, whatever law or laws would follow from one, looked upon as a cause, would follow from the other.

SECT. II.

CENTRIPETAL FORCES, FINDING THE LAWS OF FORCES, ELEMENTS OF THE ORBITS, AND MOTION OF APSES OF ORBITS, ETC.

PROPOSITION I. *Theorem.*

If a body describe any curve by means of a force constantly tending towards a centre, the line joining the said body and the centre will sweep over areas proportional to the times of description.

Let $ACEGI$ . . .*fig.* 2, represent any curve, and Q the centre towards which the force is constantly directed, and let us suppose that AB is the direction and velocity of the body when at A. Then being at A, suppose another force be impressed upon it in the direction AQ of such an intensity, that by the time the body arrives at B it would be carried to the curve over a distance equal to BC, parallel to AQ. Join AC, which will by *Lemma* 1, *Cor.* 1, vol. I, p. 43, represent the velocity and direction of the body by the combined operation of the two forces. Produce AC to D so that $CD = AC$, and D would be the point at which in a second equal time the body would arrive at D. But if when at C it received a second impulse in the direction CQ sufficient to carry it a distance equal to DE parallel to CQ, it will arrive at E by the joint action of the two forces having described the line CE. In the same way it may be traced on to G, I, &c., other points in the curve.

But the triangle $ABQ =$ triangle $ACQ =$ triangle $CDQ =$ triangle $CEQ =$ triangle EGQ, &c. That is, in equal times equal triangles by the motion of the body and the radius from Q will be described. And if the intervals of time be indefinitely reduced, the number of impulses will be indefinitely augmented, the forces sink into a continued force, and the series of lines described merge into the curve $ACEGI$. Therefore in the curve $ACEGI$ the areas swept over by the radius vector are proportional to the times of description. This is one of the laws discovered by Kepler.

Cor. 1. If the point C approaches indefinitely near to A, the line AC becomes a tangent to the curve at the point A. The same is the case with EC at the point C, EG at the point E, &c. So that the motion of the body, if the centripetal force were to cease at any instant, would be a tangent to the curve at that instant.

Cor. 2. Hence if p denote the perpendicular on the tangent at any point in the curve, and v denote the velocity of the body in the curve at that point, pv is a constant quantity, and v varies as $\frac{1}{p}$.

Cor. 3. Hence, also, the transverse velocity is inversely as the distance from the centre, and the angular velocity inversely as the square of that distance.

PROPOSITION II. *Theorem.*

If two bodies start at equal distances from any centre of force, with equal velocities, the one to describe a curve and the other a right line to or from the centre, the velocities of the two bodies will be equal at all equal distances from the said centre.

Let us suppose both bodies to start from the point A (*fig.* 3) with equal velocities, the one to describe the curve AF, and the other the line AC directly towards the centre C of force. Take any point b and draw about C the circle Bb and very near to it the other circle Dd, cutting the radius and curve in B, b and D, d, and the radius Cb in e.

Then if F be the force at B and b, its efficacy in the direction bd will be $F \times$ cos angle $dbC = F \times \frac{be}{bd}$. But the increment of velocity acquired in describing bd is equal to the intensity of the force in that direction, and the time of describing it, that is, $= F \times \frac{be}{bd} \times \frac{bd}{V} = F\frac{be}{V}$, where V is the velocity of description at b. In the same way it is found that the increment of velocity in descending from B to D $= F\frac{BD}{v} = F\frac{be}{v}$ in which v is the velocity at B. If, therefore, the velocities V, v be equal the increments will be equal; that is, the velocities at A being equal, they receive equal additions in running over spaces which equally approach the centre of force *C*; and therefore the velocities are equal in the curve and in the right line at all common distances from the centre. I believe this elegant property is the discovery of Newton.

Cor. 1. Hence, if a body start from a point of rest and descend by the force of gravity down any curve *ABC* (*fig.* 4), the velocity of a body descending by the same force of gravity down the right line *abc*, and descending from a point of rest at *a*, in the same horizontal line *A a,* will have the same velocities at all corresponding horizontal levels, *b, c,* &c., as the body in the curve has at *B, C,* &c. This supposes that the body descends the curve without friction.

The same is true up or down any mixt lines, whether of curves or right lines.

Cor. 2. Let *ACB* (*fig.* 5), be a cycloid of which *AS, SB* are the evolutes, and as it is known are equal cycloids; and let *SF* be a pendulum describing the said cycloid. Then if *HCE* be the generating circle, it is demonstrated by writers on the properties of this curve that the arc *CF* is equal to twice the chord *CE,* meeting *FD* parallel to *HB;* and that this chord is parallel to the tangent *FG.* If then *HC* denote the accelerative force of gravity, *CE* is the accelerative force in the curve at *F.* That is, the accelerative force is always as half the arc *CF* to be run. Hence by § 16, vol. I, p. 20, the times of vibration are the same, whatever portion of the curve *CB* is described.

Again, by *Cor.* 1, present lemma, the velocity acquired in descending the curve *BC* from a state of rest, is equal to the velocity acquired by freely falling from rest through HC half the length of the pendulum SC. But by (4), § 12, Book I, p. 17, if g be the force of gravity, the velocity acquired in falling through HC is $V=\sqrt{2g\times HC}=\sqrt{g\times SC}$.

Moreover, because $CB=CS$, and the curve at B is perpendicular to AB, the time of describing BC is the same as it would be to describe SC with the force $g\frac{x}{SC}$, x being any height above C. But by § 16, Book I, p. 19, that time is found to be $\frac{\pi}{2\sqrt{b}}$, where $b=\frac{g}{SC}$. Consequently, since we find by experiment that the length of a pendulum vibrating seconds in our latitude is 39·13929 inches = 3·2616 feet, we have

$$\frac{\pi}{2}\sqrt{\frac{SC}{g}}=\frac{1^s}{2},$$

or

$$g=\pi^2\times SC=32{\cdot}1906 \text{ feet},$$

the force of gravity at the surface of our earth in the latitude of 51° 31′·1 that of London.

The distance a body could freely fall through in one second is just half this.

Cor. 3. Because the radius of curvature at the lowest point C is the same as that of a circle whose radius is SC, the times of vibrating in very small arcs of a circle are the same as those of vibrating in a cycloid.

This elegant method of deducing the effect of gravity on falling bodies, is due to Huygens, one of the most distinguished philosophers of the 17th century.

PROPOSITION III. *Theorem.*

If a body moving about a céntre be attracted directly towards it by a force, the intensity of which is always as some function of the distance from the said centre, the path of that body will lie all in one plane, and will either have no apsis at all, or it will have one or two apses, but not more.

By an apsis is meant a point at which the radius vector and the tangent of the curve are perpendicular to each other. These points are obviously maxima and minima points.

That the path lies in one plane, follows from the demonstration of Proposition I.

Then with respect to the apsis. If we suppose the body projected in an obtuse angle to the radius vector, with a velocity so great that the force should not sufficiently deflect it from the rectilinear course, the force may diminish so fast that the body may run out to infinity, and not be turned sufficiently from its course to be brought back, in which case it will have no apse. For since, by Proposition II, the velocities in the curve, and in a right line passing through the centre of force, are always equal at equal distances from the centre, if they are once equal, the truth of this position may be made apparent by imagining a body to start off in a right line from the centre of force, with a velocity which could not be destroyed at an infinite distance.

Assuming the velocity not greater than could be destroyed within a finite distance, if ascending in a right line, the body must have an apse at less than that distance. Now supposing it had, it is evident that the curve described in approaching and leaving the apse is precisely the same. For by supposing the body to start back again from the apse with the velocity with which it arrived, it must have the same accelerations and same deflections at the same points, the force being the same, which it had in descending, that is, it must retrace the path it previously described.

This being the case, the body after passing the apse will either run on to infinity or it will come to another apse. If it run out to infinity, of course the path will have but one apse. Let us, however, suppose that the force and velocity are such that it will descend to a second apse. Then, for the same reason we gave above, the path of the body after leaving the second apse will be precisely similar to that which it described in coming down to it. That is, the body will reascend to an equal height it did before, and again descend, and so on *ad infinitum*.

The points to which the body ascends, or the upper apses, may not coincide, though they will all be at the same distance from the centre of force. Similarly, the lower apses may not be at the same point, but they will all lie in the circumference of a circle whose centre is the centre of force.

Hence therefore more than two apses an orbit cannot have, but it may have one or none.

Cor. 1. If two consecutive upper apses coincide, the two lower will likewise coincide, and the body will describe a recurring curve, or one continually returning into itself. In this case the angle subtended by the radii vector connecting the two apses, will be equal to two right angles.

Cor. 2. Should the angle between two consecutive apses, that is the lower and the upper, be any even submultiple of four right angles, still the curve will be a recurring curve, but not otherwise.

Cor. 3. If at the extremity of the radius vector a tangent to the curve be drawn, and a perpendicular be let fall from the centre of force on that tangent, the perpendicular and the radius vector will be equal at the apses, but at no other points. At all others the perpendicular will be less than the radius vector.

PROPOSITION IV. *Theorem.*

The centripetal force necessary to retain a body revolving uniformly in a circle about any centre of force, is equal to the square of the velocity divided by the radius of the circle, both being reckoned in the same *denomination, and the velocity being the space passed over in a unity of time.*

Let V be the velocity in the circle, R its radius, and suppose t the time of describing some very small arc AD, *fig.* 6. Draw AB a tangent to A; draw BD meeting it and the curve parallel to AC the radius; also draw DF parallel to BA; and join ED.

Then F being the force retaining the body in the circle, if we suppose v the velocity that would be acquired at the point D by the body freely descending by the force F from B to D, we shall have by (4), § 12, Book I, p. 17,

$$v^2 = 2F \times BD.$$

And since by (2) and (3), F being constant, $v = tF$, we have $t^2F = 2BD$.

But, in the same time t that the versed sine AF of the arc AD is descended, the arc itself, being supposed indefinitely small, is described by the velocity V; that is,

$$t^2V^2 = AD^2 = 2R \cdot BD.$$

Whence $$F = \frac{V^2}{R} \quad . \quad . \quad . \quad . \quad . \quad (1).$$

V is measured by the same unit that R is, and by any arbitrary unit of time, that is, either by a second, minute, hour, day, &c. F of course is measured by the same unit R is, adapted to the unit of time in V.

Cor. 1. If $F = ntg$, g being $= 32{\cdot}19$ the force of gravity, and t any number of seconds, n being any given number, we have

$$ntg = \frac{V^2}{R} \quad . \quad . \quad . \quad . \quad (2),$$

in which V is measured in feet to the time t as unity, and R is measured in feet.

In railway trains V is commonly reckoned in miles per hour, and the radius R in chains of 66 feet each. If V then be miles per hour $\frac{22V}{15}$ will be feet per second. Putting therefore $t = 1$ and substituting $\frac{22V}{15}$ for V and 66 R for R, we shall have

$$n = \frac{22^2 V^2}{15^2 \times 66R \times 32\frac{1}{6}} = \frac{\cdot 00101324 V^2}{R} \quad . \quad . \quad (3)$$

taking $g = 32\frac{1}{6}$ as usually reckoned.

Cor. 2. The circumference of the circle being $2\pi R$, if P be the periodic time,

$$P^2 = \frac{4\pi^2 R^2}{V^2} = \frac{4\pi^2 R}{F} \quad . \quad . \quad . \quad (4)$$

and

$$V = \frac{FP}{2\pi} \quad . \quad . \quad . \quad . \quad (5).$$

Moreover, if we suppose A the double area described in the time t,

$$A = tVR = t\sqrt{R^3 F} = t\,\frac{F^2 P^3}{8\pi^3} \quad . \quad . \quad . \quad (6)$$

by (4). In unity time $t = 1$.

As (4) is general, it will serve to find the periodic times of a pendulum describing a conical surface. Let A (*fig.* 7) be the point of suspension, and $AB = S$ the length of the string. Then F being the centrifugal force of the body at B, $F = \frac{4\pi^2 R}{P^2} = \frac{4\pi^2 S \sin \angle A}{P^2}$. But $F = G \tan A$ where G is the force of gravity in AC expounded as above. Hence

$$P^2 = \frac{4\pi^2 S \cos A}{G} = \frac{4\pi^2 . AC}{G}.$$

That is, the time of revolution depends upon the height of the cone, and has nothing to do with the length of the string AB or radius BC.

Because from (3) vol. I, p. 17, we get $2x = Ft^2$ when F is constant, if t be put equal to P we shall have $4\pi^2 . AC = 2x$, or

$$x : 2AC :: \pi^2 : 1,$$

where x is the distance a body would descend by the force of gravity in the time in which the pendulum makes one revolution.

Cor. 3. If $F = QR^n$, Q being some given quantity,

$$P^2 = \frac{4\pi^2}{Q . R^{n-1}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

by (4).

If $n = -2$, the squares of the periodic times in different circles are as the cubes of the radii of the circles. If $n = 0$, that is, if the force is constant, the times are as the square roots of the radii. Lastly, if $n = 1$, the periods are the same in all circles.

To find the Law of Force in Orbits.

Cor. 4. The law of the force to retain a body moving in any curve is easily found. For by Proposition II, of this Section, the force and velocity are the same, whether the body moves in a curve or a right line; and by (3) vol. I, p. 17, we have $F = -\frac{vdv}{dx}$, x being the distance from the centre, and decreasing while v increases; in other words, the force being centripetal and attractive. By *Cor.* 2, Proposition I, v being the velocity at any point, and y the perpendicular on the tangent at that point let fall from the centre of force, $v = \frac{VY}{y}$ in which V, Y are the velocity and perpendicular on the tangent at some fixed point. $A = VY$ is therefore the double area described by the radius vector in a given time, the unit of t, whatever it is. Squaring and differentiating v and its value just found, gives

$$vdv = -\frac{A^2 dy}{y^3}. \quad \text{Whence } F = \frac{A^2 dy}{y^3 dx} \quad \ldots (8),$$

in which, as we have said, A is the double area described in the assumed unit of t.

When, therefore, we have the equation between y and x, that is, the polar equation of the curve, we know the law of the force.

It may here be necessary to remark in the fundamental equation

$$F = -\frac{vdv}{dx},$$

that if the body describe a curve, v can never be 0.

Consequently F depends for its character and value on the relation between dv and dx. While these differentials have contrary signs, F is a centripetal or positive force, but a centrifugal or negative if they have like signs; that is, if they both increase or decrease at the same time. When $dv = 0$, and dx has an existence, $F = 0$.

It is evident from the equation

$$v = \frac{VY}{\mathrm{y}},$$

that dv and $d\mathrm{y}$ must always have different signs; and therefore when $d\mathrm{y}$, dx have the same signs, F is positive or centripetal, and negative or centrifugal when they have different signs.

These observations will be found to be of service hereafter, in the right understanding of some points.

We will here illustrate the theorem with a few examples.

Example 1. Let us suppose the curve to be an ellipse, the force being placed in one of the foci.

By the property of the ellipse

$$\mathrm{y}^2 = \frac{b^2 x}{2a - x} = \frac{p}{2x^{-1} - a^{-1}},$$

in which a, b, p are the semi major axis, minor axis, and parameter respectively. Hence

$$F = \frac{A^2 a}{b^2 x^2} = \frac{A^2}{p x^2} \quad . \quad . \quad . \quad (9);$$

that is, the force varies reciprocally as the square of the distance.

This is the second of Kepler's laws found out by experiment, and subsequently demonstrated by Newton. At the time it was considered to be of so difficult a nature as to baffle Halley and other eminent mathematicians.

Example 2. Suppose the centre of the force coincides with the centre of the ellipse.

The equation of the curve is now

$$y^2 = \frac{a^2b^2}{a^2 + b^2 - x^2},$$

which gives

$$F = \frac{A^2x}{a^2b^2} \quad . \quad . \quad . \quad (10);$$

that is, a force varying directly as the distance.

Example 3. Let the curve be an hyperbola, with the force in one focus.

The equation of the curve is

$$y^2 = \frac{b^2}{2ax^{-1} + 1} = \frac{p}{2x^{-1} + a^{-1}},$$

which will obviously give the same law as in the ellipse with the force in one focus.

Example 4. When the force is placed in the centre of the hyperbola, the law by the same process will come out the same as when placed in the centre of the ellipse, that is, a force varying directly as the distance from it. But for the reasons before assigned it will be a repulsive force.

Example 5. Let the equation be that of a parabola, namely,

$$y^2 = px,$$

which will give

$$F = \frac{A^2}{px^2},$$

the same as before.

If we suppose the force to be at the farther focus, that is at an infinite distance, x becomes infinite, and therefore for F to have any finite value at the upper end A too must be infinite. In this case the value of F is constant for any finite motion of the body. But since the focus to which the body tends is infinitely distant, the lines inclosing any area A must be parallel. Therefore a body describing a parabola by a force acting in parallel lines must be urged by a uniform force; and the velocity perpendicularly to the line of attraction is also uniform.

Example 6. Let the force be supposed to be in the circumference of a circle, and it be required to find the law by which the body may be made to describe the circle.

The polar equation is

$$y = \frac{x^2}{2a}$$

a being the radius. This gives

$$F = \frac{8a^2 A^2}{x^5} \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

that is, a centripetal force varying inversely as the 5th power of the distance.

Example 7. If the curve be a spiral whose equation is $a\mathrm{y}=x$, the force is

$$F=\frac{a^2A^2}{x^3},$$

varying reciprocally as the cube of the distance.

Example 8. Generally, if the equation of the curve be

$$\mathrm{y}=\beta x$$

in which βx denotes any function of x,

$$F=\frac{A^2d\mathrm{y}}{\mathrm{y}^3dx}=\frac{A^2d_x\beta x}{\beta x^3} \quad . \quad . \quad . \quad . \quad . \quad (12).$$

From the Law of Force to find the Orbits.

Cor. 5. Conversely, we may now reverse the process, and the law and value of the force being given we can find the curve the body will describe. For let the force be represented by

$$F=-A^2d_xfx=\frac{A^2d\mathrm{y}}{\mathrm{y}^3\,dx}$$

by (8), we have by integrating

$$2\mathrm{y}^2=\frac{A^2}{B-\int Fdx} \quad . \quad . \quad . \quad . \quad (13)$$

in which B is the constant of integration to be determined.

It is here necessary to notice a condition that has been mentioned in Proposition III, *Cor.* 3, namely, that the perpendicular y on the tangent must always be less than x the radius vector. If the polar equation of the curve does not give that under any particular circumstances, the equation so resulting must be rejected as absurd or impossible. This is a condition which requires particular attention to avoid erroneous conclusions, and to reconcile certain apparent anomalies.

Example 1. Suppose $F = \odot x^{0}$ same constant quantity, the curve is

$$2y^2 = \frac{A^2}{B - \odot x}.$$

To determine the character of this curve we must observe that at each apsis $y = x$, Proposition III, *Cor.* 3, which gives the general equation for determining the number of apses,

$$2x^2 (B - \odot x) = A^2.$$

We have seen by Proposition III, that the orbit cannot have more than two apses. Let the distances of these two apses, the upper and lower, then be denoted by a, b, and we shall have the two equations for determining the nature of the curve.

$$2a^2 (B - \odot a) = A^2$$

$$2b^2 (B - \odot b) = A^2$$

from which we find

$$A^2 = 2 \odot \frac{a^2 b^2}{a + b} \quad \text{and} \quad B = \odot \frac{a^2 + ab + b^2}{a + b}.$$

Hence the equation of the curve is

$$y^2 = \frac{a^2 b^2}{a^2 + ab + b^2 - (a + b)x} \quad . \quad . \quad . \quad (14)$$

From this equation it is plain that the curve must always have two apses. For if we suppose one of these distances, as a, to be infinite, $y = b$ indicating that the orbit must either be a circle or a right line. This is perfectly reasonable; for no body unless it had an infinite velocity could run off to an infinite distance with a constant force soliciting it to a point.

We may show this in the following manner.

Putting F for the force which would cause the body to revolve in a circle at the distance a with the same velocity V, as the body describing the orbit has at the apsis a, we have by (6) $a^2V^2 = a^3F$ and therefore

$$A^2 = a^3F \quad . \quad . \quad . \quad . \quad (15)$$

generally. Equating this with the value of A^2 just found, we get

$$(a+b)aF = 2\odot b^2, \text{ and } b = \frac{aF}{4\odot}\left\{1 \pm \sqrt{\frac{8\odot}{F} + 1}\right\}.$$

Of the two values here indicated to b, the positive one only can be taken, there being no such thing as a negative apse to a continuous curve. When $\odot$ is equal to F the above equation gives $b = a$; but less when $\odot$ exceeds F, and greater when $\odot$ is less than F.

Example 2. Let $F = \odot x$ or the force be directly as the distance. Then

$$y^2 = \frac{A^2}{2B - \odot x^2}$$

and
$$a^2(2B - \odot a^2) = A^2$$
$$b^2(2B - \odot b^2) = A^2$$

from which we get

$$A^2 = a^2b^2\odot, \text{ and } B = \frac{1}{2}\odot(a^2+b^2),$$

and

$$y^2 = \frac{a^2b^2}{a^2+b^2-x^2},$$

an equation to the ellipse having the force in the centre and the semi-axes equal to a, b.

Equating the value of A^2 here found with that in (15), and interpreting $\odot$ by $\frac{\odot}{a}$ to let $\odot$ represent the force at the apse a, we obtain

$$b = a\sqrt{\frac{F}{\odot}}$$

for the relation between the axes of the ellipse.

Example 3. Suppose the force be $\frac{\odot}{x^2}$ we obtain

$$2y^2(B+\odot x^{-1}) = A^2$$

from which, proceeding as before, we find

$$2(a^2B+\odot a)+A^2,$$

$$2(b^2B+\odot b)=A^2,$$

$$A^2 = \frac{2ab\odot}{a+b}, \text{ and } B = -\frac{\odot}{a+b},$$

giving for the equation of the curve

$$y^2 = \frac{abx}{a+b-x};$$

which is an equation to the ellipse where ab is equal to the square of the conjugate semi-axis, and $a+b$ is equal to the major axis.

Interpreting here $\odot$ by $\odot a^2$ this new $\odot$ is the force at the upper apsis. Putting therefore the value of A^2 just found, as affected by this interpretation, equal to the value of A^2 in (15), and we have

$$b = \frac{aF}{2\odot - F} \quad . \quad . \quad . \quad . \quad . \ (16).$$

As long as $2\odot$ is greater than F, the value of b is positive and finite; and the curve has two real apses; but when $2\odot = F$, $b = \infty$, and the equation of the curve sinks to

$$y^2 = ax,$$

the equation to the parabola.

When $2\odot$ is less than F, b is negative, and the equation of the curve becomes that of the hyperbola.

Hence, with a centripetal force varying inversely as the square of the distance, the body can describe no other curve than one of the three conic sections, which will be an ellipse, a parabola, or hyperbola, just as twice the force at one of the apses exceeds, is equal to, or falls short of the force necessary to make a body at the same distance, and with the same velocity, describe a circle.

Example 4. Let the force be $= \odot x^{-3}$, which furnishes

$$\begin{aligned} y^2(2B + \odot x^{-2}) &= A^2, \\ 2Ba^2 + \odot &= A^2, \\ 2Bb^2 + \odot &= A^2. \end{aligned}$$

From the last two equations it appears that $a = b$, and B is indeterminate. Therefore the curve may have an apse or it may not; but if it has any, it can have but one apse.

If the body start in an oblique line to the radius vector and $B=0$, the equation is that of the common proportional spiral, that is, $y^2 = \frac{A^2x^2}{\odot}$.

Assuming that the curve has an apse at the distance a, and treating the equation with regard to the value of A^2 and $\odot$, as in the preceding examples, we arrive at the equation

$$y^2 = \frac{a^2F}{\odot a^2x^{-2} + (F - \odot)},$$

in which F is the force in the circle at the distance a, and $\odot$ the force in the curve.

It is evident here that the less $F - \odot$ is, the nearer the curve approaches to a circle, that is, the closer the threads of the spiral in the successive revolutions approach each other, or the more compressed the spiral is. If $\odot$ exceed F, the spiral, starting from the apse, is all within the apse, and the body, in every revolution, approaches nearer and nearer to the centre of force. On the other hand, if $\odot$ is less than F, the spiral, starting from the apse, lies without the apse, and the body recedes farther and farther from the centre.

Hence a body may descend from infinity in a spiral, pass an apse, and go on circumrevolving in a spiral towards the centre.

Example 5. Take the general equation $F = \odot x^n$, and we have

$$2a^2\{\overline{n+1}\,.\,B - \odot\, a^{n+1}\} = \overline{n+1}\,.A^2$$
$$2b^2\{\overline{n+1}\,.\,B - \odot\, b^{n+1}\} = \overline{n+1}\,.\,A^2$$

from which we get

$$B = \frac{\odot\,\{a^{n+3} - b^{n+3}\}}{\overline{n+1}\,.\,(a^2 - b^2)}, \quad . \quad . \quad . \quad . \quad . \quad (17).$$

and $$A^2 = \frac{2a^2b^2(a^{n+1} - b^{n+1})\odot}{\overline{n+1}\,.\,(a^2 - b^2)}$$

We have also by (15) and putting $\odot\, a^{-n}$ for $\odot$, where the new $\odot$ denotes the force at the apse,

$$\frac{F}{\odot} = \frac{2b^2(a^{n+1} - b^{n+1})}{\overline{n+1}\,.\,a^{n+1}(a^2 - b^2)} \quad . \quad . \quad . \quad . \quad (18);$$

from which the ratio of the forces F, $\odot$ being given at the apse a, and the power n, the relation between a and b may be found, when there is a second apse.

This equation cannot be solved generally, except between the limits $n = 3$ and $n = -5$, and then with the exception of $n = -4$. Its form, however, is so simple, that the relation between a and b may be easily had tentatively, to any degree of accuracy, for all values of n. So, likewise, it may when $a - b$ is very small.

The general equation of the orbit is of a simple character, that is,

$$y^2 = \frac{a^2b^2(a^{n+1} - b^{n+1})}{a^{n+3} - b^{n+3} - (a^2 - b^2)x^{n+1}} \quad . \quad . \quad . \quad . \quad (19).$$

Example 6. If $n=-1$, the above equation becomes a vanishing fraction, and sinks to

$$y^2=a^2b^2\,\frac{\log\frac{a}{b}}{\log\frac{a^{a^2}x^{b^2}}{b^{b^2}x^{a^2}}}.$$

Example 7. Suppose $n=-5$ then (19) becomes

$$y^2=\frac{a^2+b^2}{1+a^2b^2x^{-4}}.$$

If one of these quantities, as b, be assumed infinite with regard to the other, the equation sinks to

$$ay=x^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (20),$$

which is the equation to the circle, of which a is the diameter, and the force is at a point in the circumference.

From (18) we likewise obtain

$$b^2=\frac{a^2F}{2F-\odot}.$$

Consequently, if F is less than $\frac{1}{2}\odot$, the equation is impossible, because b is the square root of a negative quantity. If $2F=\odot$, one of the apses is infinitely distant compared to the other. The polar equation of the curve always gives y less than x, between the values $x=a$ and $x=b$, and is therefore legitimate; but the equation (20) will give y greater than x, unless x is equal to or less than a. Therefore, in this particular case, a is the higher apsis, and the condition of one apsis being infinitely greater than another, is fulfilled by the other being actually in the path of the body.

It is, however, plain from the polar equation, that the circle is not the only curve which may be described by a force varying inversely as the 5th power of the distance. The circle is merely a simple case of the general problem.

Cor. 6. By the preceding *Cor.* we determine the elements of the orbit, when one of the apses, and the velocity, and intensity of the force at that apsis, are given; but the elements of the orbit may be found by having the direction, velocity, and intensity of the force at the extremity of any radius vector given. For if q be the value of y in the particular position at which the body is projected, and p be that of x, we obtain from (13)

$$2q^2(B-\int fp.dp)=A^2 \quad . \quad . \quad . \quad (21),$$

in which the quantity A is known from the velocity and q, and the quantity $\int fp.dp$ is given; so that the only unknown quantity is B, which therefore becomes known. Hence when $x=a$ we have given B just found, with A, and the form of $\int fa.da$, from which the value of a may be determined, and thence, by the preceding *Cor.*, the species and elements of the orbit.

Periodic Times in Orbits.

Cor. 7. We may now proceed to find the periodic times in recurring orbits. The periodic time P is evidently equal to the whole area of the orbit divided by $\frac{1}{2}A$ the area swept over in a unity time. Therefore if C be the area of the curve,

$$P = \frac{2C}{A}.$$

Example 1. The area of the ellipse is $C = \frac{\pi ab}{4}$, in which a, b are the axes. But when the force is $\odot x$, an ellipse is described, by Example 2, *Cor.* 5, having the force in the centre, and $A^2 = \frac{a^2 b^2 \odot}{16}$.

Consequently

$$P = \frac{C}{\frac{1}{2}A} = \frac{2\pi}{\sqrt{\odot}} \qquad . \qquad . \qquad (22).$$

That is, the periodic times are the same in all ellipses great or small, about the same force in the centre; and about different forces the periods are in the inverse subduplicate ratio of the masses of the attracting bodies.

Example 2. If the force be in one focus, as in Example 3, *Cor.* 5, $A^2 = \frac{b^2 \odot}{2a}$. Therefore

$$P^2 = \frac{C^2}{\frac{1}{4}A^2} = \frac{2\pi^2 a^3}{\odot} \qquad . \qquad . \qquad (23).$$

Or the squares of the periodic times are as the cubes of the major axes of the ellipses.

This is another of the great laws discovered by Kepler.

If the ellipses are described about different bodies, the squares of the times are as the cubes of the major axes directly, and the masses of the attracting bodies inversely.

Example 3. We have seen by Example 7, *Cor.* 5 that a circle may be described with a force varying as the 5th power of the distance inversely, provided the force be placed at some point in the circumference of the circle. In this case $C=\frac{\pi a^2}{4}$; and supposing b infinite, which gives the case $A^2=\frac{\odot}{2a^2}$

and $$P=\frac{2C}{A}=\frac{\pi a^3}{\sqrt{2\odot}}.$$

Consequently the periodic times of circles so described are as the cubes of their diameters directly and the square root of the masses of the attracting bodies inversely.

Of the Velocities in Orbits.

Cor. 8. Since $\frac{A}{\mathrm{y}}$ is the velocity v at any point of the orbit distant x from the centre of force, if a, b be the semi-axes, and P the periodic time, we have

$$\mathrm{y}v=A=\frac{2\pi ab}{P} \quad . \quad . \quad . \quad . \quad (24).$$

Therefore when $\mathrm{y}=b$ we get

$$v=\frac{2\pi a}{P} \quad . \quad . \quad . \quad . \quad . \quad (25).$$

That is, the force being anywhere in the major axis, the velocity at the extremity of the minor axis is directly as the major axis, and inversely as the periodic time, without any regard to the law of the force.

As the periodic times of all bodies describing ellipses with the force in one focus are the same, provided the major axis and mass of the force are the same, it follows that at the extremities of the minor axes the velocities are the same, and equal to the velocity of a body describing a circle at an equal distance, about the same force.

Hence also the periodic times of bodies describing different ellipses, are as the major axes directly, and the velocities at the extremities of the minor axes inversely, whatever be the law of the force, or wherever placed in the major axes.

Cor. 9. Since $v\mathrm{y} = A^2$, and the general value of A is given in (17), and of y in (19), the value of v, the velocity of the body in the orbit, can be found at all distances x from the centre of attraction in terms of x, and the apsidal distances.

Cor. 10. If v denote the angular velocity of a body at the distance x we have $\mathrm{v}x^2 = A = \frac{2\pi ab}{P}$, and

$$\mathrm{v} = \frac{2\pi b}{aP},$$

when $x = a$. That is, if the species of the ellipse, or ratio of its axes be given, the angular velocity at a distance equal to the major semi-axis, will always be inversely as the periodic time, no matter what the size of the ellipse be, by what law of force the body is kept revolving in its orbit, or whereabouts the centre of force is situated. The same is the case when the body is at a distance equal to the minor semi-axis from the force.

Angle between the Apses.

Cor. 11. The angle between the apses, or the angle described from either apsis may be found. For if we put dz for the differential of that angle, xdz is the transverse velocity of the body at the extremity of the radius vector x, and

$$xdz : \pm\, dx :: \mathrm{y} : \sqrt{x^2 - \mathrm{y}^2} :: 1 : \sqrt{\frac{x^2}{\mathrm{y}^2} - 1}$$

Therefore,

$$dz = \frac{dx}{x\sqrt{\frac{x^2}{\mathrm{y}^2} - 1}} \quad . \quad . \quad . \quad . \quad (26)$$

the right-hand number being always positive, because the quantity under the radical changes its sign as dx does, by falling on different sides of the radius vector x. By the help of (19) we get the value of the fraction $\frac{x^2}{\mathrm{y}^2}$ in a function of x, and thence by integration the value of the angle z, from either apsis.

The integration of the right-hand member being between the limits $x = a$ and $x = b$ may be eluded by the use of definite integrals, but the result will be given in a series. Since also $\frac{\mathrm{y}}{x}$ is the sine of the angle between the radius vector and the tangent, the integral may be found in terms of that sine.

PROPOSITION V. *Theorem.*

If a smaller body revolve about a larger by the force of attraction, and the attraction be mutual and equal, the larger body will not be at rest, but both bodies will describe similar curves about their common centre of gravity.

For if the attraction be mutual, the intensity of attraction on either will be as the mass of the attracting multiplied into the mass of the attracted body, and therefore will be the same on each. But the accelerative force will be as the force of attraction directly, and the mass of the body inversely. The momentary fractions of the paths, therefore, described will be facing each other, and perfectly similar; and hence the whole paths will be similar, but in dimensions inversely proportional to the masses of the bodies describing them. Consequently, the centres of these orbits will be one and the same point in the radius vector, and dividing that radius in the ratio of the masses of the two bodies. That is, it will be at their common centre of gravity.

Cor. Hence the common centre of gravity of two bodies revolving about each other, is either at rest or moves uniformly forward in a right line.

PROPOSITION VI. PROBLEM.

It is required to find the forces of the sun which disturb the moon's motion.

If the sun always attracted the moon by the same force as it does the earth, and in parallel directions, the moon would move round the earth, during the latter's annual motion about the sun, the same as if the earth was at rest. But as the moon is sometimes nearer and at other times farther from the sun than the earth is, it is attracted by a greater or a less force, and its motion consequently is subjected to disturbances.

Let S (*fig.* 8) denote the sun, E the earth, and M the place of the moon at any time while revolving in its orbit HMA. Join EM, MS, and let Se represent the sun's attraction on the earth at E. Draw em, eh respectively parallel to EM, EH. Then if ☉ denote the mass of the sun, ☾ that of the moon, and we put $SE=a$ and $EM=r$, we have

$$Se=\frac{\odot}{a^2}, \text{ and } hm=\frac{\odot r \sin z}{a^3},$$

in which z represents the angle HEM. Putting

$$Sq=\frac{\odot}{SM^2}=\frac{\odot}{(a-r\sin z)^2},$$

for the attraction of the sun on the moon at M, we have

$$hq=\frac{2\odot r \sin z}{a^3}=2hm,$$

when r is very small with respect to a.

Draw qp parallel to ME or me, and the force Sq

may be resolved into the forces *qp*, *pS*, from which taking the force *Se* retaining the earth in its orbit, and which acting alone would not disturb the moon's motion about the earth as before observed, leaves *qp*, *pe* for the direction and magnitude of the disturbing forces on the moon's motion. But *r* being very small with regard to *a*, the force in the direction of the earth's radius is

$$pe = qm = \frac{3\odot r \sin z}{a^3};$$

and the other

$$qp = me = \frac{\odot r}{a^3}.$$

Of these forces the latter always acts in the direction of the earth's attraction on the moon, and therefore augments that attraction, but the former, which is parallel to the line joining the centres of the sun and earth, acts, in the inferior half of the moon's orbit, in the direction of the sun's attraction on the earth, and in the superior half in the opposite direction.

Cor. 1. By (23), if *E*, *M* be the periodic times of the earth and moon in their orbits

$$\frac{E^2}{M^2} = \frac{a^3 \rightmoon}{r^3 \odot} = \frac{af}{rF}$$

F, *f* being the attractions on the earth and moon to keep them in their orbits at their mean distances. Hence if we suppose $F = a$

$$f : r :: E^2 : M^2 \quad . \quad . \quad . \quad . \quad (27)$$

that is, the force which retains the moon in her orbit about the earth, is to a force denoted by the radius of her orbit, as the square of the earth's periodic time is to the square of the moon's,

$$: : (365{\cdot}2564)^2 : (27{\cdot}32166)^2 : : 178{\cdot}7233 : 1 \ldots (28).$$

We have here supposed that the earth and moon are each kept in their orbits by forces varying inversely as the squares of the distances; but the same equation (27) results from (4), by which the bodies are supposed to move in circles and no particular law of the force is assumed.

Cor. 2. The force 178·7233 above found, is not the earth's attraction, but the mean force compounded of the earth's and mean of the sun's. From this, however, we can find the earth's mean attraction. Produce me, *fig.* 8, to meet a perpendicular from p at n. Then en is the part of the force ep acting in the radius of the moon's orbit, and is always opposed in direction to me. But ep being in the mean state of affairs equal to $3MN$, and the angle pen being the complement of the angle hem or NEM, we have

$$en = 3MN \sin \angle NEM = \frac{3}{r}(r^2 - EN^2).$$

Taking this from r, the force in me, leaves

$$\frac{3EN^2 - 2r^2}{r} \quad . \quad . \quad . \quad . \quad (29)$$

for the force acting contrary to the moon's gravity towards the earth.

Now since the force $r - \frac{3MN^2}{r}$ from which (29) is derived, is always measured in the direction of MN, the area of a curve whose ordinate is the function (29) divided by the base $HE = r$, will give the mean ordinate or mean force acting on the moon during one quadrant, equivalent to the mean action of the two forces ME and $3NM$, in the radius of the moon's orbit. By multiplying (29) by $-d.NE$, and integrating from $NE = r$ to $NE = 0$, and dividing by r, we find this mean force equal to $-r$, or by (28) equal to -1.

The mean force, therefore, of the earth on the moon is to the disturbing force ME,

as 179·7233 to 1 . . . . (30).

It is in the computation of this mean disturbing force in the lunar radius vector, that Newton has fallen into the error which gave the mean motion of the moon's apses only about one half the true amount. I have on that account been somewhat more minute than I should otherwise have been, in pursuing the computation.

Cor. 3. We may in the same way calculate the mean value of the force pn acting perpendicularly to the lunar radius vector. Its value is

$$\frac{3NM \, . \, NE}{r},$$

and therefore may be taken with regard to NE or

NM. Computed either way it will come out r, the same as the preceding mean force.

This force accelerates the moon's tangential velocity during the 2d and 4th quadrants, and diminishes it during the 1st and 3d. Therefore the moon's tangential velocity is accelerated in the former cases by a $\frac{1}{179{\cdot}7233}$ part of the earth's attraction, or by a $\frac{1}{178{\cdot}7233}$ of the combined force retaining the moon in her orbit, and diminished by as much in the latter cases.

Cor. 4. If the moon describe a circle, the amount of the acceleration of the velocity, by the tangential force acting from the quadratures to the syzygies during the fourth of a synodical revolution, may easily be computed. Calling the mean tangential force unity, and v the increment of velocity acquired in any time t from the quadrant, we have, by (1), vol. I, $v = t$. But the tangential force being unity, the centripetal force retaining the moon in her orbit is $F = 178{\cdot}7233$, and by (1) $V^2 = RF$. So that

$$\frac{v}{V} = \frac{t}{\sqrt{RF}} = \frac{2\pi t}{PF}$$

by (4). Therefore if $t = \frac{p}{4}$, or one fourth of a synodical revolution (29·530588 days), and $P (= 27{\cdot}32166)$ a sidereal revolution,

$$\frac{v}{V} = \frac{\pi p}{2PF}$$

is the fraction of V generated in a fourth of a synodical revolution. Hence

$$V+\tfrac{1}{2}v : V-\tfrac{1}{2}v :: 1+\frac{p\pi}{4PF} : 1-\frac{p\pi}{4PF} :: 4PF+\pi p :$$

$$4PF-\pi p :: 19624{\cdot}8428 : 19439{\cdot}2950 :: 1 : {\cdot}990591$$

is the ratio of the velocity of the moon in the syzygies to her velocity in the quadratures, supposing she moved in a circle and was acted on by the tangential force to accelerate her velocity. Her momentary areas are therefore in this ratio. Newton has computed this ratio by following the tangential force through all its variations, to be equal to the ratio of 11073 to 10973, which is equal to that of 1 to ·99097, differing very little from that which we have given by computations from the mean force.

PROPOSITION VII.

If PQ, fig. 9, *be any quiescent curve described by a body, and a similar curve revolving about C be described, the difference between the centripetal forces retaining the body in the revolving and quiescent orbits, will vary reciprocally as the cube of the distance from the centre of force C.*

Suppose A to be any position of the body at the end of the time t. Then if the orbit was at rest during some small time dt, the body would be found at B say, at the end of it; but instead of that it is found at b, a point in the circle about C passing through B, having described the curve Ab. Draw the radii CA, CB, Cb. It is then evident that AB, Ab will be equal to the velocities of the bodies in the quiescent and revolving curves, drawn into the time dt; and DB, Db will be as their transverse velocities. But since, by *Cor.* 3, Proposition I, p. 265, the transverse velocities are inversely as the distance from the centre, $DB : Db :: 1 : r$ a constant ratio.

Again, in the little triangles ADB, ADb, which being in their nascent state may be treated as rectilinear,

$$\begin{aligned} Ab^2 &= AD^2 + bD^2 = AD^2 + r^2 . DB^2 \\ AB^2 &= \qquad\qquad\quad\; AD^2 + \;\; DB^2 \end{aligned}$$

Whence $Ab^2 - AB^2 = \overline{r^2 - 1} . DB^2$. But Ab, AB being respectively equal to Vdt and vdt, in which V, v are the velocities in the revolving and quiescent orbits, and $F_{,}$ F being the forces at A in the direction of AC by which the revolving and quiescent orbits are described, we shall have

$$(VdV - vdv)dt^2 = \overline{r^2 - 1} . DB . dDB =$$

$$\overline{r^2 - 1} . \frac{Adt}{x} \times - \frac{Adxdt}{x^2},$$

where $dV = F_{,} \frac{AD}{Ab} dt = F_{,} \frac{AD}{V}$, and $dv = F \frac{AD}{AB} dt = F \frac{AD}{v}$, and $AD = - dx$. Therefore

$$F_{,} - F = \overline{r^2 - 1} . \frac{A^2}{x^3} \quad . \quad . \quad . \quad . \; (31).$$

Cor. 1. Hence

$$r = \sqrt{(F_{,} - F)\frac{x^3}{A^2} + 1} \quad . \quad . \quad . \quad (32).$$

Cor. 2. If we interpret F by $F\frac{a^2}{x^2}$; that is, if the body describe a conic section about one focus, and F is the force at the distance a, we have, for the force in the revolving orbit,

$$F_{,} = \frac{Fa^2}{x^2} + \frac{r^2 - 1}{x^3} A^2 \quad . \quad . \quad . \quad (33).$$

PROPOSITION VIII. *Theorem.*

If fx, any function of the distance x, represent the centripetal force by which a body describes a curve, the law of that force, at any instant in any other radius vector y, *is* $\frac{dfx.\mathrm{y}}{fx.d\mathrm{y}}$.

For suppose F the force, and that when F increases by dF, y increases by dy; then if we put $\mathrm{y}^n \propto F$,

$$F : F + dF :: \mathrm{y}^n : (\mathrm{y} + d\mathrm{y})^n :: \mathrm{y}^n : \mathrm{y}^n + n\mathrm{y}^{n-1}d\mathrm{y} + \,..$$

and $F : dF :: 1 : n\frac{d\mathrm{y}}{\mathrm{y}} + n\,.\,\frac{n-1}{2}\,.\,\frac{d\mathrm{y}^2}{\mathrm{y}^2} + \ldots$

or $F : \mathrm{y} :: dF : nd\mathrm{y} + n\,\frac{n-1}{2}\,\frac{d\mathrm{y}^2}{\mathrm{y}} + \ldots$

which gives

$$n = \frac{\text{y}.dF}{F.dy} = \frac{\text{y}.dfx}{fx.dy} \quad . \quad . \quad . \quad . \quad . \quad (34).$$

Cor. 1. Hence the following

RULE. *The law of any force in any radius vector, is equal to the differential of the hyperbolic logarithm of the force divided by the differential of the hyperbolic logarithm of the radius vector.*

Cor. 2. A force which varies as $1 + x + \frac{x^2}{1\,.\,2} + \frac{x^3}{1\,.\,2\,.\,3} +$ is as x^x. This is easily shown.

Cor. 3. Having the law of a force given, we may decompose it into its constituent forces. For example, if $F \propto x^{\frac{1+x}{1-x}}$, we may assume it equal to $A + Bx^r + Cx^{r+1} + Dx^{r+2} + \ldots$ the law of which being found by the Rule *Cor.* 1, and equated with $\frac{1+x}{1-x}$, gives, putting $A = 0$,

$$r = 1,\ C = 2B,\ D = 3B,\ E = 4B, \ldots$$

Whence the force is

$$F = B\,\{\,x + 2x^2 + 3x^3 + \ldots\}$$

that is, the sum of an infinite series of forces, varying as x, $2x^2$, $3x^3$, $4x^4$ . . . .

Cor. 4. Bodies are made to describe similar orbits, when the laws of the forces by which they are retained in those orbits are similar. Now the force of a body to describe an ellipse with its apses moving in consequentia, is by the preceding Proposition, $a^2Fx^{-2} - a^3Dx^{-3}$, where $D = \overline{r^2 - 1}.A^2\,a^{-3}$, the law of which, by the present Proposition, is

$$\frac{-2a^2Fx - 3Da^3}{a^2Fx + Da^3} = -2 - \frac{Da}{Fx + Da} \quad . \; . \; . \quad (35),$$

in which F is the force causing the body to describe the quiescent ellipse at some distance a, and should be expounded by $F\frac{a^2}{x^2}$; and D at that time is equal to $\overline{r^2 - 1}.A^2\,a^{-3}$

The law, therefore, for the apses of an ellipse to move forward, must vary in a greater than the inverse duplicate ratio of the distances, and for them to move backwards, it must vary in a less than the duplicate inverse ratio.

Cor. 5. Let the force be $a^2Fx^{-2} + \theta x^n$, whose law by the present Lemma is

$$-2 + \frac{\overline{n+2}.\theta a^{-2-n}x^{n+2}}{F + \theta a^{-2-n}x^{n+2}} \quad . \quad . \quad . \; (36).$$

Equating this law with (35), we get, after substituting for D its value $\overline{r^2 - 1}.A^2a^{-3}$, and for A^2 its value Fab^2 drawn from (9), (F in this value being interpreted by $F\frac{a^2}{x^2}$) by due reductions,

$$r^2 - 1 = -\frac{\overline{n+2}.\theta a^{-n}x^{n+3}}{b^2(aF + \overline{n+3}.\theta a^{-1-n}x^{n+2})} \quad . \quad . \; (37).$$

Cor. 6. If a, b, and x are sensibly equal; that is, if the orbit differs very little from a circle it becomes

$$r^2 - 1 = -\frac{\overline{n+2}.\theta}{F+\overline{n+3}.\theta} \text{ or } r^2 = \frac{F+\theta}{F+\overline{n+3}.\theta} \ldots(38)$$

which, when $n = 1$ and $\theta = -1$, sinks to

$$r^2 - 1 = \frac{3}{F-4} \text{ and } r = \sqrt{\frac{F-1}{F-4}} \quad . \quad . \ (39).$$

In this case, therefore, the apses move forward.

By substituting the number 179·7233, found for the earth's mean force on the moon, we have 1° 31′ 48″ for the mean progressive motion of the moon's apses during a half sidereal revolution, supposing her orbit to be very nearly a circle. The tables give 1° 31′ 19″.

Hence the theoretic quantity exceeds by 29″ the observed. This is owing chiefly to the moon describing about the earth, by her mean motion, not a circle, as we have assumed, but an oval, having the earth in the centre, and the conjugate axis always pointing to the sun, that is, lying in the syzygies. For as in the syzygies the earth's attraction on the moon is diminished by twice the radius, and in the quadratures increased by the radius, the force is greatest in the quadratures and least in the syzygies, at an equal distance from the earth, and the moon consequently describes a curve receding farther from the earth in the former case than her mean distance, and approaching nearer in the latter. The force, therefore, which disturbs the moon's motion in the syzygies, and which causes the apses to move forward, is less than it should be; while the force which operates in the quadratures and causes a retrograde motion of the apses, is greater than it should be.

Another cause of a contrary character lies in the eccentricity of the moon's orbit. The disturbing force is computed for the moon's mean distance from the earth, that is, when the moon is at the extremities of the conjugate axis of her ellipse. But the ratio of the disturbing force to the earth's is greater the greater the moon's distance from the earth, on two accounts, one because the radius vector, to which the disturbing force is proportional, is greater, and the other because the earth's attraction is less as the radius vector increases. The reverse happens when the moon is nearer her lower apsis than her mean distance. Now, since the time of description is longer, because the area to be described is greater in the upper half of her orbit, where the disturbing force is greater, than it is in the lower, where the said disturbing force is less, the total excess of the disturbing force above the mean quantity is more during the moon's transit through the upper half, than the defect is less than the mean quantity during her transit through the lower half; that is, the mean disturbing force throughout a whole revolution is greater than at the moon's mean distance from the earth, and of course the total mean motion of the apses is greater.

The difference of the above two causes makes up the trifling difference between the observed and the computed quantities of the apsidal motion.

This subject of the motion of the apses is introduced merely in consequence of the celebrity which for many years it attained by the defect in Newton's computation. The method, however, here followed, in all the computations by the mean forces or otherwise, is merely approximative, and cannot be compared, in point of accuracy of results, with the direct solution of the problem of the three bodies, as obtained by the French or English mathematicians. Our object has, indeed, not been any attempt to rival those processes, for that would be almost absurd, but simply to show that there is no defect in the old method, introduced by Newton, of explaining the lunar inequalities, but that the error lay with him in taking a wrong mean force.

SECT. III.

ON THE HEAT, ATTRACTIVE ENERGIES, ETC., OF THE PLANETS.

Proposition I.

If several bodies of the same kind, at various distances from the sun, be heated by the emanations alone of that body, emitted equally every way about, the temperatures of these bodies, after an indefinite long time, will be the one to the other reciprocally as the distances of the bodies from the sun.

For after a very long period the bodies, whatever be their nature, must acquire such a temperature that their radiations will just counterbalance, and be counterbalanced by, the heat they are every moment receiving from the sun. But this heat being emitted in right lines every way about, the quantity which momentarily falls on any given surface is reciprocally proportional to the square of the distance from the heating centre. This, therefore, is the rate of radiation, that is, abstracting the quantity received, it is the fluxion of the temperature in terms of the distance. Consequently, calling x the distance and t the total temperature,

$$dt \propto \frac{1}{x^2} \text{ or } dt = -\frac{adx}{x^2},$$

a being a constant depending on the amount of solar emanations. Hence, integrating

$$t = \frac{a}{x} + \text{constant}.$$

But since at an infinite distance t must $= 0$, it follows that this constant of integration is null; and therefore

$$t = \frac{a}{x}.$$

Cor. 1. Because the radiation of any body is proportional to its surface, and the rays from the sun impinging on it are likewise proportional to the same surface, the size of the body has no effect on the equality subsisting between the heat radiated and heat received, nor therefore on the conditions of our theorem, which hence holds true, whatever be the relative size of the bodies.

Cor. 2. Hence the absolute temperatures of bodies moving round the sun in circles would be to each other reciprocally as the radii of their orbits; for their being in motion or at rest makes no difference.

Lemma.

The areas described in equal times by any two planets revolving round the sun are in a ratio compounded of the simple direct ratio of the minor axes, and the inverse subduplicate of the major axes.

Let A, $A_{,}$ denote the areas of the ellipses, X, $X_{,}$ the major, and Y, $Y_{,}$ the minor semi-axes, and P, $P_{,}$ the periodic times; then

$$A : A_{,} :: YX : Y_{,}X_{,}$$ by the property of the ellipse,

and $$P : P_{,} :: X^{\frac{3}{2}} : X_{,}^{\frac{3}{2}},$$

by (23), Book IV, Section 2. Consequently

$$\frac{A}{P} : \frac{A_{,}}{P_{,}} :: \frac{Y}{\sqrt{X}} : \frac{Y_{,}}{\sqrt{X}};$$

and $\frac{A}{P}$, $\frac{A_{,}}{P_{,}}$ are evidently the areas described by each body in a unity of time. Therefore the areas described, &c.

Cor. If the area $A_{,}$ belong to a circle $Y_{,} = X_{,}$, and if we suppose the radius $= 1$,

$$\frac{A}{P} : \frac{\pi}{P_{,}} :: \frac{Y}{\sqrt{P}} : 1.$$

PROPOSITION II.

If a body revolve round the sun in an ellipse, the mean momentary quantity of rays falling on the body in an entire revolution is reciprocally as the rectangle of the axes of the ellipse.

Let $Q =$ quantity of rays fallen on the body in T time;
$X, Y =$ as before, the semi-axes;
$x =$ variable distance of the body from the emanating centre;
$\theta =$ the angle described in the time T;
$a =$ the rays received in unity time at unity distance;
$A =$ the area described in the time T.

Then it is evident that

$$dQ = \frac{adT}{x^2}.$$

But by *Cor.* to the preceding Lemma,

$$dA = \frac{\pi Y dT}{P_{,}\sqrt{X}}.$$

And it is manifest that

$$dA = \frac{x^2 d\theta}{2}.$$

Multiplying the first of these equations by the third and dividing by the second, we have

$$dQ = \frac{aP_{,}\sqrt{X}}{2\pi Y} d\theta;$$

which integrated is

$$Q = \frac{aP_{,}\sqrt{X}}{2\pi Y}\theta;$$

and for the whole circumference 2π,

$$Q = \frac{aP_{,}\sqrt{X}}{Y}.$$

Let this be divided by the period $P = P_{,}X_{,}^{\frac{3}{2}}$ and we have for the mean momentary quantity of rays received in unity time,

$$\frac{a}{XY}.$$

We have not here troubled ourselves with correcting the integral, since it is easily seen that the constant must be nothing:

Cor. 1. Hence the mean momentary radiation or quantity of rays is reciprocally as the area of the ellipse.

Cor. 2. The mean momentary radiation of a body moving in an ellipse, is the same as that of a body moving in a circle of equal area. For since by the preceding *Cor.* the mean radiation is as the area only of the ellipse, it matters not how the species of the ellipse changes, if the area remain the same; and therefore if the ellipse degenerate into a circle of equal area, the mean radiation will be the same.

PROPOSITION III.

The mean temperature of a body describing an ellipse about the sun, and deriving its heat from no other source, is the same as that of a body describing a circle of equal area.

This Proposition might well have been put as a corollary to the last, but from its importance it is fully entitled to a separate enunciation.

By *Cor.* 2 to the preceding Proposition the mean radiation in a body describing an ellipse is the same as that of a body describing an equal circle. But as the radiation depends on the temperature, and is greater or less as that is greater or less, it is plain that two bodies which have the same radiation must have the same temperature.

Cor. Hence it follows that the mean temperatures are as the square roots inversely of the areas in an entire revolution, or as the square roots of the rectangles of the axes of the ellipses inversely.

PROPOSITION IV.

If two bodies of equal quantities of matter revolve about the sun, the intensities of their attractions are reciprocally proportional to the square roots of the areas of their ellipses.

For by the preceding Proposition and its corollary the mean temperatures are as the square roots of the areas inversely, and by 3d Law, *Cor.* 1, Proposition Book IV, Section 1, p. 251, the attractions are as the temperatures; therefore the attractions are in the reciprocal subduplicate ratio of the areas, or products of the axes.

PROPOSITION V.

In any two bodies moving about the sun, their attractive forces are directly as their quantities of matter, and reciprocally as the square roots of the areas of their ellipses jointly.

For when the temperatures are the same, the attractions are directly as the quantities of matter, and when the quantities of matter are the same, the forces, by the preceding Proposition, are inversely as the square roots of the areas. Therefore, when neither the quantities of matter nor temperatures are the same, the forces are in the simple direct ratio of the quantities of matter, and the reciprocal subduplicate of the elliptic areas conjointly.

Cor. 1. If the bodies are similarly constituted, their attractive forces will be very nearly as the square roots of the areas inversely, and the cubes of the diameters of the bodies directly. I say very nearly, because, though the bodies be precisely the same in constitution, yet, in consequence of their expansion and contraction by increase and diminution of temperature, the same quantity of matter is not contained under the same volume.

Cor. 2. Hence if the bodies are similarly constituted, the attractive forces of equal volumes are very nearly as the square roots of the areas inversely.

Cor. 3. Granting the same things, the attractions of the planets at their surfaces, are very nearly as the square roots of the areas of their orbits inversely, and the diameters of the planets directly. For, other things alike, the attractions at the surfaces are as the diameters directly.

Cor. 4. Hence in the planets the attractions at their surfaces are nearly in the compound ratio of their mean distances inversely, and their diameters directly.

Scholium.

The preceding Propositions have been investigated on the supposition that the radiating centre is a mere point. With regard to our sun; though its magnitude is very small compared to the distance of the planets, it is not quite correct to neglect it altogether. But the difficulty is to make a proper estimate of its radiation in different parts of its surface. The following Proposition, by M. Fourier, if founded on correct data, will enable us to do this; and it is on the presumption of its accuracy, which can only be determined by constating it with good experiments that the three next Propositions can be depended on.

PROPOSITION VI.

The intensity of radiation from any surface in a perpendicular direction, is to that in an oblique, as radius to the sine of the angle which the said oblique direction makes with the surface.

Let *CA* (*fig.* 10) be a part of the surface of the body, *CB*, a very small depth from which all the radiating particles come; that is, so that from depths exceeding *CB* no radiating particles reach and pass through the surface. Again, let *AIB* be a curve, such that the ordinate *EI*, at any point, *E*, expresses the intensity of radiation from this point perpendicular to the surface; and the mixtilinear area *BIAC* will clearly be the whole intensity of radiation from the line *CB* perpendicular to the surface, or in fact the perpendicular radiation from the point *C*. In order to find the total intensity of radiation in any oblique direction *EFJ*, take *CL* equal to *EF*, draw *LH* the ordinate, and *HM* parallel to *BC*, cutting the ordinate *EI* in *G*. Then, if the obstacles to free radiation rise merely from the depth or thickness of substance which the rays have to pass through, the radiation of the point *E*, through the point *F*, will be the same as that of *L* through *C*, and will therefore be as *LH* or *EG*. Consequently, if *EF* move parallel to itself from or towards *C*, and *CL* increase or diminish with *EF*, *G* will be always a point in the curve *AGL*, and *EG* the intensity of the radiating point E through the surface in a direction parallel to FJ. Hence the area $DGAC$ will be as the total radiation in the direction FJ. But $MG : MH :: CE : CL :: CE : EF$ in every part of the curve, and therefore the whole area $DGAC$: the whole area $BIAC :: CE : EF$. That is, the total radiation in a direction perpendicular to the surface, is to the total radiation in the direction FJ, as CE to EF, or as the sine of the angle JFA to radius.

PROPOSITION VII.

Granting the same law of radiation, it is required to find the radiation from a spherical surface on any point at a distance from it.

Suppose P (*fig.* 11) the point on which it is required to estimate the total radiation, from the spherical superficies of which DEA is a section. Draw from any point D the tangent DB, sine DC, and radius DS. Then if $PS = p$, $DS = r$, and the angle $DSP = z$, we have by the preceding Proposition $a \times \frac{\sin PDB}{PD^2}$ for the radiation of the point D or P, supposing a a constant representing the force of perpendicular radiation of unity surface at unity distance. Multiplying this value by $2\pi . CD = 2r\pi \sin z$, and substituting for PD^2 its value $(PS - SC)^2 + CD^2 = p^2 - 2rp \cos z + r^2$, and observing that $\sin PDB : \sin DBP$, or $\cos z :: PB$ or $p - \frac{r}{\cos z} : PD$, we have

$$2a\pi r^2 \frac{(p \cos z - r) \sin z}{(p^2 - 2rp \cos z + r^2)^{\frac{3}{2}}} dz$$

for the differential of the radiation of the surface, of which *DAF* is a section, on the point *P*, whose integral is

$$-\frac{2a\pi (p - r \cos z)}{\sqrt{p^2 + r^2 - 2rp \cos z}}.$$

And if this be taken from $\cos z = 1$ to $p \cos z = r$, it will give

$$2a\pi \left\{1 - \frac{1}{p}\sqrt{p^2 - r^2}\right\} = 2a\pi \left\{1 - \sqrt{1 - \frac{r^2}{p^2}}\right\} \ldots(1)$$

for the whole radiation of the sphere on the point *P*:

Cor. When $p = \infty$ the expression vanishes, and the radiation is 0; and when $p = r$ it is barely $2a\pi$.

PROPOSITION VIII.

Required the temperature of a body revolving round the sun in a circle at the distance p, things remaining as in the last Proposition.

If x be the distance from the sun's centre, we get by the preceding Proposition, $2a\pi \left\{1 - \sqrt{1 - \frac{r^2}{x^2}}\right\}$ for the radiation. And, according to the principles of Proposition I, multiplying this by $-dx$ and integrating, which gives

$$2a\pi\left\{-x+\sqrt{x^2-r^2}+r\sin^{-1}\frac{r}{x}\right\} \quad . \; . \; . \; . \quad (2),$$

we shall have the temperature of the body, supposing it to have revolved for an indefinitely long period. For as the expression vanishes when $x = \infty$, it evidently wants no correction.

General Scholium.

It is usually considered that bodies attract in exact proportion to their quantities of matter, and that therefore when a body of equal volume attracts with a greater force than another, it is taken for granted it possesses more matter, or, as it is termed, is more dense. According to our views, this is true only when the bodies are of the same temperature. Consequently, in comparing our deductions with the notions usually entertained, our attractive forces, in bodies similarly constituted, are nearly the same as the ordinary densities. If the bodies are not similarly constituted, then the quantity of matter, multiplied by its temperature, corresponds to the ordinary estimation of density.

In applying the preceding Propositions to such bodies as our planets, we may utterly disregard the diameter of the sun, even at the distance of our earth. For though the earth's distance be only about 210 of the sun's semidiameters, yet such is the trifling influence this would have on the earth's temperature, as calculated by (2), that it would not alter the ratio of it, if computed from a radiating point, a 1-1,000,000th part. The same observation might be made in regard to the little influence of the eccentricities of the planetary orbits. Even in the orbit of Mercury, the most eccentric of all, the neglect of the eccentricity would not affect the calculated density by a 1-400th part.

Our computations, therefore, of the temperatures and densities of the planets, are, from these circumstances, considerably simplified. By the latest observations, it would seem the mean distances of the planets are

☿	♀	⊖	♂	♃	♄	♅
3871	7233	10000	15237	52028	95388	191824;

and therefore their true temperatures are

2·583	1·383	1·	·656	·192	·105	·052.

These would likewise be their proportional densities, were it not for the circumstance of expansion and contraction. As bodies grow colder, they contract in their dimensions, so as to contain a greater quantity of matter under the same volume; and, on the contrary, the warmer they are the more they expand, and thus consequently inclose a less quantity. If, therefore, our computations are made without regarding these contractions and expansions, it is evident that the densities of the superior planets will come out less than they ought, while those of the inferior will be greater, merely because the matter in the former will be less, and in the latter greater, than is assumed in equal volumes of our earth. But with what materials are we to compare these immense globes? What is the rate of expansion for such ranges of temperature? And how, therefore, are we to make due allowance for the effects of temperature? Questions like these are difficult to answer. Immersed in such a dilemma, we should not probably err much in comparing the expansions and contractions with those of that substance, of which at least the greater part of the earth's surface is composed, namely, water. Not knowing anything to the contrary, we may likewise assume that the expansions and contractions are, for greater ranges within reasonable limits, equable and proportional to that of water (namely, $\frac{1}{21\cdot5}$) between the temperatures of 32° Fahr. and 212°.

Now if we suppose the true temperature of the earth to be about 1020 in our tables, which corresponds to 51°$\frac{1}{5}$ Fahr., the temperatures of these bodies will be

	☿	♀	⊖	♂	♃	♄	♅
true,	2635	1411	1020	669	196	107	53,
Fahr.	2884°	507°	51°	—233°	—429°	—443°	—446°.

The enormous difference between the temperatures of the Earth and Mercury, amounting to 2833°, seems to defy all attempts at correction, inasmuch as it is extremely unlikely that the law of expansion which we have for 180°, should hold good for 15 times that range. Besides, as it does not appear that the mass and density of Mercury have been made the subject of direct observation, but only determined from analogy, it would be only wasting time to endeavour to make accurate computations. We shall therefore begin with Venus. The temperature of this star is 450° above ours. Therefore $\frac{450}{180} \times \frac{1}{21\cdot5} = \frac{1}{8\cdot6}$, nearly; and if we diminish 1·38 by a $\frac{1}{8\cdot6}$th, it will give 1·22 for the theoretic density of Venus. Observation gives about 1·25, that of the earth being 1. Mars's difference of temperature is 284, and $\frac{284}{180} \times \frac{1}{21\cdot5} = \frac{1}{13\cdot6}$. Adding, therefore, to ·656 a 2-27th, it gives about ·704, and observation ·70. Jupiter differs in temperature by 480°, and $\frac{480}{180} \times \frac{1}{21\cdot5} = \frac{1}{8}$, nearly, which applied as in Mars, to ·192, makes ·216 for the density of ♃, while Laplace gives ·218. So Saturn's difference is 494°, which will occasion a deviation so little different from 1-8th, the amount for Jupiter, that it may be safely used. Hence increasing ·105 by 1-8th, or a little more, gives ·119 for the density of ♄, that of Laplace being ·126. The allowance, likewise, for ♅ is about the same as that for ♄, and makes ·060, being much within ·29, which is assigned for its density.

Brought together, the observed and computed densities will stand thus:

	☿	♀	⊖	♂	♃	♄	♅
Computed	not comp.	1·22	1	·704	·216	·119	·06
Observed	not obser.	1·25	1	·70	·218	·126	·29

In the 5th vol. of the 'Minutes of the Royal Astronomical Society,' p. 134, the following densities of the sun and planets are given by M. Encke, in a letter to the Astromomer Royal.

☉	☿	♀	⊖	♂	♃	♄	♅
0·25	1·12	0·92	1·00	·095	0·24	0·14	0·24.

These numbers are determined from the supposed effects of the several planets to account for the errors in the motions of Encke's comet of short period. It need hardly be observed, that such a method is beset with difficulties of too formidable a nature to give results to be relied on even under the skill of such men as M. Encke. Mr. Airy determines the mass of ♃ at ·23, that of the Earth being 1.

We see above the agreement of theory with observation. One exception, or rather not so close an agreement, appears in the new planet Herschel. Whether this body's density has been accurately determined; whether the body is somewhat differently constituted from the other planets; or whether it be really a new member (in its present place) of our system, is doubtful. If the first be the case, which, considering its distance and the difficulty of making the observations, is not unlikely, better observations will probably annihilate the difference; if the second, it makes not against our theory; inasmuch as we contemplate a perfectly similar constitution, leaving the contrary equally possible. Should the third supposition be true, namely, that the planet has not always circulated in its present orbit, it is highly probable that future observations will establish a variable density.

According to this view of the cause of what is called density, that of the moon, if she be of the same nature as our earth, should be equal to the earth's, because she is at the same mean distance. Laplace (Systême du Monde, chap. 10, vol. ii, on the tides) says it is ·840, that of the earth being 1; and Newton (Principia, Book iii, Proposition 37, Cor. 3), makes it 1·22. The mean of these is 1·03, very nearly what it ought to be by our theory and the hypothesis of a similar constitution. Both these philosophers made their computations from the same phenomena, the tides. With respect to the mass of the moon, and of course its density, great uncertainty exists. M. Poisson says, (Connaissance des Tems, for 1836, p. 63) that the mass of the moon varies from a $\frac{1}{69}$th of the earth's to a $\frac{1}{90}$th, according to the phenomena employed and the observations used. The phenomena are the tides, the lunar equation of the sun's apparent motion, and the nutation of the earth's axis, either of which Poisson thinks equally proper. This difference in a body so near us, whose effects are so manifest, and therefore, as one might suppose, so little liable to error, is great; but great as this is, it is moderate compared to the difference of some, whose estimations of the lunar density disagree no less than 100 per cent.!

One curious fact seems to result from the above calculations, namely, that all the visible planetary bodies of our system are very similar in their constitution; and differ little or nothing from our earth.

From the theory here advanced it follows, that the attractive forces of comets must necessarily be very weak. These bodies are usually not only very small in magnitude, but the extent of their excursions exposes them to a depression of temperature which very much diminishes their centripetal forces. Halley's comet, which used to be considered "the Mercury of comets," revolves at a medium 17 times farther from the sun than the earth, and therefore by our theory would have only about the 1-17th of the earth's energy. Consequently, if this be joined to an inferiority of magnitude, it might produce effects scarcely cognizable. Other comets which move in larger orbits, would of course be still more marked in their effects. The comets of 1472 and 1760, which came very near the earth, that of 1454, which is said, though erroneously, to have eclipsed the moon, and the one in 1770, which passed in the midst of Jupiter's satellites, without either of them having produced the least appreciable effects, are proofs of the weakness of comets' attractions, but not, as some imagine, of their being deficient in matter. These bodies may not want matter, though from the vast distances at which they revolve, their temperatures may be exceedingly low, and their attractive forces consequently very feeble. A comet, however, appeared in 1828, which it seems would argue an inconceivably small portion of matter, in respect to its volume. This comet which at present has a period of only $3\frac{1}{3}$ years, is known by the name of Encke's comet. From a difficulty of reconciling former observations on it with later, Encke was led to conclude, that its matter was so insignificant as to subject its motions to be affected by what had heretofore been considered the resistless ether of the heavens. Indeed this deficiency of matter has been otherwise proved, namely, by the appearance of a small star, of the eleventh magnitude, by Struve, through its middle; but this observation seems to be contradicted by another of M. Wartmann, at Geneva, who says he saw a star of the eighth magnitude "completely eclipsed" by it on the same evening, the 28th November, 1828.

To put his views of a resistance to the test, M. Encke previously computed and published in 1827, in the 'Connaissance des Tems' for 1830, p. 273, an ephemeris of its motions for its expected return in the fall of 1828, on the hypothesis of a resisting medium, with the differences on the hypothesis of non-resistance. Observations, it is said, confirmed the conjecture of a resistance, being only about $2'$ or $3'$ in *RA* and $1'$ in declination constantly at variance with the calculations, and always in advance of them. The difference of situation in the two hypotheses rise to near $39'$ in *RA*, and $10'\frac{1}{2}$ in declination at the maxima. While, therefore, observations verified one ephemeris within $2'$ or $3'$, they fixed errors on the other, amounting sometimes to

36′. A very probable corroboration of Encke's hypothesis is this. On a matter of less consequence it would be held to be nearly conclusive; but where so much depends, and where the conclusions are traversed by so many other phenomena, it would be prudent to pause and wait for more confirmations before a decisive opinion is hazarded.

Should a sensible resistance to the motions of this body and the like be actually established, God knows what changes it may introduce in physical astronomy!

Other observers besides M. Encke have noticed the very small quantity of matter in some comets, and have furnished facts almost demonstrating that the comets they discussed were little better than rarefied vapour. Sir John Herschel mentioned, at the meeting of the British Association at Newcastle, in 1838, some circumstances of a very striking nature respecting Halley's celebrated comet, which we cannot do better than quote in his own words, as reported in the 'Athenæum,' of September 8th, 1838, p. 648.

"One of the most interesting series of observations, of a miscellaneous kind, I had to make at the Cape of Good Hope, was that of Halley's comet. I saw the comet for the first time, after its perihelion passage, on the night of the 25th of January. Mr. Maclear saw it on the 24th. From this time we, of course, both observed it regularly. Its appearance at first was that of a round, well-defined disc, having near its centre a very small bright object exactly like a small comet, and surrounded by a faint nebula. This nebula, in two or three more nights, was absorbed into the disc, and disappeared entirely. Meanwhile, the disc itself dilated with extraordinary rapidity, and by measuring its diameter at every favorable opportunity, and laying down the measures by a projected curve, I found the curve to be very nearly a straight line, indicating a uniform rate of increase; and by tracing back this line to its intersection with its axis, I was led, at the time, to this very singular conclusion,—viz. that on the 21st of January, at 2h. p.m., the disc must have been a point—or ought to have no magnitude at all! in other words, at that precise epoch some very remarkable change in the physical condition of the comet must have commenced. Well! all this was speculation. But here comes the matter of fact I refer to, and which, observe, was communicated to me no longer ago than last month by the venerable Olbers, whom I visited in my passage through Bremen, and who was so good as to show me a letter he had just received from M. Boguslawski, Professor of Astronomy at Breslau, in which he states that he had actually procured an observation of that comet on the night of the 21st of January. Well then, how did it appear? Why, as a star of the sixth magnitude—a bright concentrated point, which showed no disc, with a magnifying power of 140! And that it actually *was* the comet, and no star, he satisfied himself, by turning his telescope on that point where he had seen it. It was gone! Moreover, he had taken care to secure, by actual observation, the place of the star he observed; that place agreed to exact precision with his computation; that star *was* the comet, in short. Now, I think this observation every way remarkable. First, it is remarkable for the fact that M. Boguslawski was *able* to observe it at all on the 21st. This could not have been done, had he not been able to direct his telescope point-blank on the spot, by calculation, since it would have been impossible in any other way to have known it from a star; and, in fact, it was this very thing which caused Maclear and myself to miss procuring earlier observations. I am sure that I must often have swept, with a night-glass, over the very spot where it stood in the morning before sunrise. And never was astonishment greater than mine at seeing it riding high in the sky, broadly visible to the naked eye, when pointed out to me by Mr. Maclear, who saw it with no less amazement, on the 24th. The next remarkable feature, is the enormously rapid rate of dilatation of the disc, and the absorption into it of all trace of the surrounding nebula. Another, is the interior cometic nucleus. All these phenomena, while they contradict every other hypothesis that has ever been advanced, so far as I can see, are quite in accordance with a theory on the subject, which I suggested on the occasion of some observations of Biela's comet, a theory which sets out from the analogy of the precipitation of mists and dews from a state of transparent vapour on the abstraction of heat. It appears to me, that the nucleus and grosser parts of the comet must have been entirely evaporated during its perihelion, and reprecipitated during its recess from the sun, as it came into a colder region; and that the first moment of this precipitation was precisely that I have pointed out as the limit of the existence of the disc, viz. on the 21st of January, at 2 p.m., or perhaps an hour or two later."

These are very remarkable circumstances, and render it probable that some, at least, of the comets are little better than collections of vapour. That may furnish one reason for their very small attraction; but a reason fully as competent may be found in their low temperature, without depriving them of mass. It is impossible in comets of a long period, that their attractions can be otherwise than very small, though they may have considerable matter.

It may be as well for us here at once to anticipate, a little more minutely than we have, an objection that may be made to our mode of correction for the difference of temperature, and to reply to it. The excess of temperature of Venus above the earth's is 450° Fahr. and the defect of ♃, ♄, and ♅'s nearly 500°. Now if these bodies be composed of such materials as our water, with the expansion and contraction of which we have compared them, they would, with a heat far inferior to that of Venus, from what we know of water, be rapidly dissipated; and with a heat hundreds of degrees above that of ♃ or even of ♂, be converted into hard solids. In either case the law of expansion and contraction which holds good in water, would not be followed. Arguing in the usual way this is strictly true. But though the particles of the several planets may be of precisely the same size, in which case the laws of expansion and contraction would be the same, does it follow that they must likewise be of the same figure? And it is the figure of the particles which regulates the period of their solidification, and of the component atoms, in like manner, which governs their decomposition. If the particles in some cases and the atoms in others be adapted, as no doubt they are by the great Architect, to the situations they occupy, the various planets may have particles of exactly the same size, and therefore following the same laws of expansion and contraction as the particles of any material on the earth does, and yet fulfil all the conditions their situation requires.

But let it be again distinctly understood that there is nothing in our theory which demands even this equality of size in the particles of the planets. Should they be so, we have computed what ought to be their attractive energies volume for volume with our earth. The coincidence of the calculations with phenomena, except in the ambiguous case of Herschel, certainly seem to say that we have fallen on the track nature has taken. If it be so, then, by following it out, we shall be able to pry more closely, than eyes or glasses would allow us, into the internal constitution of the planetary bodies.

We have assumed the mean temperature of our globe to be about that of the latitude we occupy. There is no doubt but this is too low, and a higher mean should be chosen. Our numbers would then correspond still better with the details of experimental facts. We have also assumed the dilatations and contractions equable and proportional to water's, between the limits of water freezing and boiling. For such extreme temperatures one would imagine this is straining observations beyond their natural tension. Probably it would be in matters of great nicety; but in ours, where the corrections are not very delicate, the error which the effect of 40° or 50° would occasion is not of material moment. And in any case a departure from the law assumed would appear much sooner in high than in low temperatures; and hence become much more sensible in the inferior than in the superior planets.

Just as the above, which was written some fifteen years or more since, was preparing for the press, a planet beyond Herschel, was discovered in a most singular manner by M. Leverrier, after whose name the planet was, by a sort of common consent, at first called.

It is not our intention to take part in the discussion respecting the two claims advanced for a participation in the honour of this extraordinary event, it being now generally admitted that nothing can interfere with M. Leverrier's undoubted right to the discovery—a discovery for which all ought to join their congratulations, and for which we as Englishmen have more particularly cause to be proud, as having furnished the means, through our immortal Newton, for making it. Mr. Adams himself, we are glad to see, has subscribed to the claims of M. Leverrier. It is thus men should act; they lose nothing in the estimation of the world by generous concessions to rivals and competitors. We advert to the subject for a different purpose. It appears that the Leverrier has a period of about 217 years, and therefore moves through a sign of the zodiac in 18 years, at the rate of $1\frac{2}{3}$ degree per year. Herschel describes a sign in 7 years, with a motion of $4^\circ\frac{2}{7}$ per year. Therefore Herschel's annual relative motion to Leverrier is $2^\circ\frac{13}{21}$. In August last year Herschel's heliocentric longitude was nearly 12°, and Leverrier's say 326°, which gives Herschel 46° in advance. Consequently, about $17\frac{1}{2}$ years back, or the beginning of 1829, Herschel and Leverrier were in conjunction; and about 35 years ago Herschel was just as much behind Leverrier as he is now before him, that is, a sign and a half. For 35 or 40 years, therefore, Leverrier, whose disturbances on Herschel enabled his discoverer to detect him, has been joining his forces to Herschel's, to disturb the motions of Saturn and Jupiter. May not the effects of this united action, or the greater portion of them, have been fathered on Herschel; and hence have caused the mass and density of that planet to be overrated?

If it be so, it is probable that the knowledge of the existence of Leverrier will enable astronomers to separate the effects of these two planets, and to bring

Herschel within the law which governs the density of the other planets. I am not aware how the mass of Herschel has been computed, but I presume it is from his effects on Jupiter and Saturn. If it has been from the periods and elongations of any of his satellites, it would probably afford less scope for uncertainty.

Mr. Airy feels confident, by his observations on Herschel about the quadratures, that his radius vector is considerably too small. The rectification of that, therefore, would tend to diminish the observed density of Herschel.

If it should turn out that Herschel's density obeys the same law as that of his companions in the system, it will throw a new light over the mechanics of the heavens, and may lead one nearer to a discovery of the origin of the planetary bodies. The Propositions and computations in this Section have this object in view, and are intended to place a few landmarks to guide further inquiries.

SECT. IV.

ON THE ALTITUDES OF THE ATMOSPHERES OF THE PLANETS AND COMETS.

PROPOSITION I.

If two bodies, similarly constituted and of equal temperatures, be enveloped with atmospheres of the same *kind, and of little altitude compared to the diameters of the bodies, the altitudes of the atmospheres are inversely proportional to the diameters of the bodies.*

We have seen, *Cor.* 4, Proposition I, Section 6, Book III, p. 141, other things being alike, that the altitudes of the atmospheres are reciprocally proportional to the attractive forces. But the attractive forces of spheres similarly constituted, are, at their surfaces, directly as their diameters. Therefore the altitudes are reciprocally proportional to the diameters.

Cor. Hence in small spheres the altitudes are greater, and, if there be only a proportional quantity of air round them, the atmospheres are rarer than the atmospheres of larger bodies.

PROPOSITION II.

If two bodies similarly constituted, having revolved round the sun for an indefinite long period, are surrounded with atmospheres of given specific gravities, it is required to find the altitudes of the atmospheres.

Putting A, $A_,$ for the areas of the orbits described; r, $r_,$ for the semi-diameters of the planets; and g, $g_,$ for the accelerative forces of the attractions at the surfaces;

$$g : g_, :: r\sqrt{A_,} : r_,\sqrt{A}, \quad \text{or} \quad g_, = g\frac{r_,}{r}\sqrt{\frac{A}{A_,}},$$

Cor. 3, Proposition V, Section 3, Book IV. Hence g being known, $g_,$ becomes known.

Moreover *Cor.* Proposition III of the same Section, if T, $T_{,}$ denote the true temperatures of the bodies,

$$T : T_{,} : : \sqrt{A_{,}} : \sqrt{A}, \text{ and } T' = T\sqrt{\frac{A}{A_{,}}}.$$

By (9), Book III, Section 6,

$$\mathbf{V}^2 = gbmT^2,$$

supposing $T = 1$ at 32° Fahr. Therefore if G, $G_{,}$ be the specific gravities of air and any other gas, and $\mathbf{V}_{,}$ the corresponding velocity, then, by the laws of gases,

$$\mathbf{V}_{,} = \mathbf{V}\,\frac{T_{,}}{T}\sqrt{\frac{G}{G_{,}}} = \mathbf{V}\sqrt{\frac{AG}{A_{,}G_{,}}} = T\sqrt{\frac{AGgbm}{A_{,}G_{,}}}.$$

But by (7), Book III, Section 6, when $V = 0$, we have for the altitude of the atmosphere,

$$x = \frac{6r\mathbf{V}^2}{gr - 6\mathbf{V}^2},$$

which, for any other planet and gas, becomes

$$x_{,} = \frac{6r_{,}\mathbf{V}_{,}^2}{g_{,}r_{,} - 6\mathbf{V}_{,}^2} = \frac{6r_{,}T^2bm}{\frac{r_{,}^2}{r}\cdot\frac{G_{,}}{G}\sqrt{\frac{A_{,}}{A}} - 6T^2bm} \quad . \; . \; (1),$$

a general expression for the altitude of the atmosphere, where $r = 3958$ miles the radius of the earth, $r_{,}$ is the radius of the new planet, G_1 the specific gravity of its gas to G that of our atmosphere, $m = 10463$, and $b = 2{\cdot}493446$.

In orbits nearly circles $\sqrt{\frac{A_{,}}{A}} = \frac{R_{,}}{R}$. We must also observe in the calculation that $r_{,}$ and r should be each expressed in feet. Consequently, to have $x_{,}$ in miles, we must divide $r_{,}$ of the numerator, so reduced to feet, by 5280, and then each being kept to miles, we have

$$x_{,} = \frac{156533\cdot58\, r_{,} T^2}{5280\frac{r_{,}^2}{r}\cdot\frac{G_{,}}{G}\cdot\frac{R_{,}}{R} - 156533\cdot58\, T^2} = \frac{r_{,} T^2}{\cdot0337308\,\frac{r_{,}^2 G_{,} R_{,}}{rGR} - T^2} \quad . \quad . \quad (2),$$

Cor. 1. We see by either expression that the greater $r_{,}$ is the less is $x_{,}$, and *vice versâ;* for the denominator will increase or decrease with the increase or decrease of $r_{,}$ faster than the numerator will. Other things, therefore, being alike, the greater the body the more contracted its atmosphere, and the contrary.

Cor. 2. $R_{,}$ and $r_{,}$ remaining the same, the less $G_{,}$ is the greater will be the altitude $x_{,}$. Indeed $G_{,}$ may be so small that the atmosphere would be infinite. It is not, therefore, every air that the body could retain about it as an atmosphere.

Cor. 3. The same things will follow from the diminution of $R_{,}$. A body may be so near the sun that the gas, which might form an atmosphere round our earth, would fly off in all directions.

Cor. 4. It is likewise plain, other things alike, that the higher the superficial temperature T, the higher is the altitude. For the increase of T increases the numerator and diminishes the denominator, both of which concur to augment the altitude. The temperature may indeed be so high as to make the denominator $=0$, and cause all the air to disperse and fly off.

Scholium.

Hence the lunar atmosphere is much higher than ours. For if we suppose the atmosphere to be of the same kind $G_{,}=G$; and because the distance from the sun is the same $R_{,}=R$. Therefore when $T=1$,

$$x_{,}=\frac{1080}{\cdot 0337308\,\frac{1080^2}{3958}-1}=120\cdot 80 \text{ miles.}$$

If the moon had the same quantity of atmosphere about it as the earth has, it would therefore be much rarer than ours.

For Jupiter we have $r_{,}=45761$, $r=3958$, $\frac{R_{,}}{R}=5\cdot 202791$, and hence $x_{,}=\cdot 49286$ miles; that is, only about half a mile. Jupiter, therefore, could scarcely be surrounded with an atmosphere of the specific gravity of our earth's; for if it was, mountains of a very ordinary height would ascend above the top of the atmosphere, and the rapidity of diminution in temperature would be a source of the greatest annoyance to the inhabitants. Such a hill for instance as Shooter's-hill, or the heights of Dover, would, from the change of temperature, be almost as formidable as the Andes in America; and the Cheviot Hills would actually rear their heads beyond the limits of the atmosphere. Indeed, with an air of this kind, the Jovial clouds would scarcely ever ascend 100 feet from the surface, which, in most places for the greater part of the year, must be involved in perpetual and impervious fogs, at least so it would be with us. An atmosphere of the levity of our hydrogen would have only about seven miles of altitude. It is, therefore, highly probable that the inhabitants of Jupiter, if there are any, must be so far different that, exclusive of the change of temperature, they could not live and breathe the air we do.

If ♃ denote the altitude of Jupiter's atmosphere and ♄ that of Saturn's,

$$\text{♃} : \text{♄} :: \frac{76068}{95388} : \frac{91522}{52028} :: \frac{22}{3} : \frac{21}{2} :: 11 : 7 \text{ nearly.}$$

So that the atmosphere of ♄ would be more unfavorable than that of ♃ for our terrestrial inhabitants. The same is true of the planet Herschel. Therefore it is highly probable that the atmospheres of these remote planets are composed of much subtiler media than our earth's is. In Mars the diameter is to our earth's, nearly as 5 to 9, and the ratio of the mean distances as 17 to 11; consequently the earth's atmosphere is to Mars's nearly as $5 \times 17 : 9 \times 11 :: 6 : 7$. The altitude of Mars's atmosphere is therefore about a sixth greater than ours. Venus's is probably half again as deep; and Mercury's much deeper still.

With respect to the comets very little can be said for want of the dimensions of their orbits and nuclei; but the following computation may throw some light on the subject. Suppose a body moving at the distance of 17·78 times the earth's distance from the sun, that is, the mean distance of Halley's comet, and to be only 200 miles in diameter in the nucleus; this body would have an atmosphere, if composed of such a one as ours is, of 194·08 miles. That is, it would be double the semi-diameter of the body. And if the air was of the specific gravity of about 10·3, hydrogen being 1, its altitude would amount to 10 times its semi-diameter, or 1000 miles. An atmosphere of the levity of our vapour would fly off in all directions and be utterly dissipated.

These computations have been made on the supposition that the earth's mean temperature is that of freezing water. If it be taken higher, it will make a difference. Generally it will give the altitudes greater.

Several consequences of the computations we have made accord very well with observations. Thus, in the superior planets Jupiter and Saturn, they are not only colder, but the diminution of temperature in their atmospheres is much more rapid and abrupt than it is in our air. The consequence is, that any vapour is much earlier condensed into clouds after it has left the planet; and hence clouds of that extent and density may be formed as to become visible at the vast distance we are from them. In Mars, the rapidity of diminution is less than with us; but the temperature, too, is less. These causes may conspire, therefore, to render Mars's atmosphere less cloudy than the two higher planets, but sufficiently so to make the clouds sometimes visible on the earth. With Venus and Mercury both the temperatures and atmospheres are higher—two causes which unite to give much clearer atmospheres, which are phenomena well known to astronomers. I believe no trace of a cloud has ever been seen in either Venus or Mercury.

In our moon the temperature is about the same as the earth's, but the atmosphere being four times higher, the limit of the condensation of vapour is much higher, and much less abrupt than with us. Hence the cause producing clouds, is likewise much less powerful. But there is another circumstance which probably contributes more effectually still to the transparency of the lunar air. We allude to the long time the sun continues to shine on any particular part of the moon. For one half of a lunar month the sun is constantly heating the same part. Now, without any reference to theory, experience teaches us, that heat will not only disperse vapours, mist, &c. already formed, but that the excess of vapour which any additional heat raises from the earth, is by no means proportional to this additional heat. Hence, though more vapour exists in the atmosphere by a long-continued sun, the air is nevertheless clearer. This is abundantly evident in our own summer and winter skies. In the former, the quantity of vapour considerably exceeds that in the latter, and yet the air is by no means so cloudy. Similarly it must be with the moon. We see the enlightened part of it only, or that in which of course the air is the clearer. The other part—which, for aught we know, may be surcharged with clouds pouring forth torrents of rain, on account of the vapour raised by heat previously received—is hid in impenetrable darkness.

According to measurements, which a few years since obtained general credit, the altitude of the lunar mountains was considered to bear the same proportion to the altitude of the terrestrial, as we have above computed the altitude of its atmosphere does to ours; that is, they were thought to be about three or four times as high. A late observer has, however, reduced the highest to about $1\frac{3}{4}$ miles, or about a third of the computed height of the Himalayan mountains. Unless, therefore, the moon's atmosphere be composed of much heavier particles than ours is, it would be impossible for any of her mountains to rise above or even up to the general region of her clouds. It is consequently hardly likely that there would be any great changes in the appearance of the lunar hills, by the melting of snow, or otherwise, from the time of their immerging into the sun's rays to that of their emerging from them.

By *Cor.* 2, we have seen that the less $G_,$ is, the greater is $x_,$, and that $G_,$ may be so small as to make $x_,$ infinite. This will be the case in our atmosphere; for putting $G=1$ in (2), $r_,=r$, $R_,=R$, and $T=1$ we have

$$G_1 = \frac{1}{\cdot 03373 \times 3958} = \cdot 00749.$$

Therefore, at 32° Fahr. and above, our earth could retain no gas in the shape of an atmosphere whose specific gravity was ·00749, or $\frac{3}{400}$, nearly, of our air, or in round numbers $\frac{10\cdot 8}{100}$, or $\frac{1}{9}$th of the specific gravity of hydrogen. May not this be a probable reason why we have no gas of so little a specific gravity? Was there an air less in specific gravity, it is not unlikely that by slow liberation it would at length be altogether lost; and the loss of such an air might have very considerable influence on animated nature. Might not an air of that or less levity once have formed a part, and a very material part of the vivifying elements of our atmosphere? and if so, might not its absence be the cause of that contraction of human life, which, on the authority of Scriptures, we are taught to believe has taken place? I put this merely as a question.

Grosser atmospheres might be retained round the superior planets, but it is manifest from the calculations we have made in this scholium, that they would be altogether unsuited to the purposes of habitable convenience. It is consequently very probable that these bodies are enveloped with airs of a very different texture; most likely they are much lighter. On the contrary, the inferior planets, both on account of their smallness and higher temperatures, ought to be encompassed with airs of a grosser quality; otherwise they would be quickly dissipated. No doubt that wisdom which is so inimitably displayed in the rest of the universe, in fitting the agents to the objects, is not wanting here.

Was a body to describe a very eccentric orbit round the sun, its atmosphere would undergo very great and manifest changes. Approaching the perihelion and for some time afterwards, the planet would constantly increase in temperature, and its atmosphere correspondingly expand. The contrary would take place in the body's recess from the sun. A little therefore after the aphelion the atmosphere would be the least, and a little after the perihelion the greatest. And were these points to be very unequally distant from the sun, the atmosphere in one case would sink nearly into the body of the planet, and in the other expand probably to many times the planet's diameter. Such would be the case was nothing else to disturb the atmosphere. But the central attraction being supposed to remain nearly the same, the excess of temperature about the perihelion would dispose the atmosphere to be much less obedient to its retaining force, and more disposed to yield to any external action, especially if the temperature approach in anywise to the limit of dispersion pointed out in *Cor.* 4. Consequently a small force, such as that of the sun's rays, would be sufficient to beat back to a considerable distance many of the exterior particles of an atmosphere so feebly retained. Of these some may possibly be driven off with force enough to be left behind or scattered into space never to return; but by far the greater number having lost the impetus given them, either by the attraction of the body, or by having escaped farther action by retreating from fresh impulses behind, will return like stones thrown up from the earth.

Among the particles so driven back a portion of light will be scattered, and reflected hither and thither in all possible directions. This will necessarily be feeble, and to a distant observer present the phenomena of a luminous train or tail.

The weaker the atmosphere is retained about the body, the nearer it approaches to the limit of dispersion, and the more numerous the solar rays are which fall on it, the greater the number will be that are driven back, and the farther the distance to which they may go. Of course the number driven back will also materially depend on the quantity of atmosphere about the body. Now the less the magnitude of the body is and the greater the mean distance of its orbit from the sun, the weaker is its attractive force; and the nearer it approaches the sun in its perihelion, the greater is the number of rays which act upon it. Therefore, other things being alike, this atmospheric train will be greater the less the diameter of the body, the greater its periodic time, and the more eccentric its orbit is. And generally it will increase in length as the body approaches its perihelion, and decrease as it recedes from it, growing broader and broader towards its extremity. But it will be at its maximum and minimum length, not exactly at the perihelion and aphelion, but when the temperature of the body's surface and action of the sun's rays combined, are the greatest and least, that is, a little after the perihelion and aphelion. It will likewise be greater after it has left the sun than it was in its approach, at an equal distance from it. For the heat of the surface being augmented, without a proportional increase of attraction, the atmosphere will approach nearer the limit of dispersion; and consequently be more obnoxious to the action of the solar rays.

Though these trains or tails will be opposite to the sun, they will not lie in a direct line. For as the angular velocity of the body about the sun is continually increasing in its descent towards the sun, and the angular velocity of any atmospheric particles will be the same as that of the body at the time of leaving it, the extreme parts of the tail must have a less angular velocity than the head, and consequently the tail be incurvated or concave towards the parts left behind. This curvature will be more apparent the longer the train is, and will increase towards the perihelion, about which it will be greatest. After the body has passed the perihelion, its velocity being continually on the decrease, the angular velocity will by little and little gain on that of the head. The tail's curvature will therefore, by degrees, grow less; and it may happen that it shall end in becoming straight or even (of which there have been instances) concave, towards the parts the comet is approaching. Generally, however, the curvature will be greater in the descending part of the orbit, unless, perhaps, just about the perihelion, than in the ascending.

Newton (Principia, b. iii,) says that the deviation of the tail from a true opposition with the sun, "appears to be less when the comet approaches the sun." If this be not merely apparent, owing to the position of the observer, it arises from the greater celerity with which the particles are driven from the head by the solar rays at the perihelion. Euler implies the contrary to Newton. To see the phenomena of a comet's tail to perfection, a person ought to be elevated high above the plane of its orbit. This, however, is a position but rarely had in any situation of a comet, and still less when at its perihelion, which, in most comets with long trains, is much nearer the sun than the earth is. They must therefore, at such times, be viewed more obliquely, which will cause the tails to appear straighter than they really are, and thus produce the phenomena Newton mentions.

Should a comet with a long train move in a very eccentric orbit, the new matter driven off from the head, particularly near the perihelion, may so far outstrip in angular velocity that previously sent off, as to be in advance of it, and nearer the true opposition with the sun. Two, three, or more tails may in this manner be formed to the same comet. Phenomena of this kind have been repeatedly observed. The comet of 1744 had a tail consisting of no less than six streams of light; and that of 1811, Delambre informs us, had two, forming an angle of from 15°

to 20°. After the body has passed its perihelion its angular velocity diminishes, the velocities of the head and extremity of the tail presently become nearly equal, and then the several tails merge again into one.

Astronomers have remarked, that the advancing parts of a comet's tail are usually more dense and luminous, and better defined than the retreating parts; and that the tail, in what may be termed its axis, is darker than at its edges, so as sometimes to present almost the appearance of two tails. These are phenomena one might expect. For any straggling particles sent off more feebly than others, will in the anterior parts be overtaken, and reinforce the main body, while in the posterior they will be left behind, and thus cause a more ragged appearance. So, likewise, the particles being driven off equally all round, they will form a sort of hollow, luminous cone, of which the preceding and succeeding parts will appear the brightest, merely because the visual lines have here the greatest numbers of these particles to pass through.

According to Aristotle, Hevelius, and others, the size and brilliancy of the head of a comet are always less when the splendour of the tail is the greatest, and the contrary. Hevelius has likewise stated that a comet's atmosphere on the side next the sun, is more compressed than on the opposite. These circumstances may very well be, since they are the obvious consequences of feeble attraction in the head. It has also been observed, that the tail is more curved, and deflects quicker from the true opposition towards the extremity, than near the nucleus, phenomena which evidently result from the angular velocities of the head and several parts of the tail being more unequal the farther they are apart.

In opposition to what has just been advanced, relative to the increasing dimensions of comet's atmospheres as they approach the sun, Hevelius, Pingré, and others, have remarked that the real diameter of the apparent nebulosity actually diminishes. For a long time this remark attracted no attention, and it was probably regarded as being chiefly the effect of accident. But the return of Encke's comet in 1828 has given it a character of much more importance. By the following table of the dimensions of the real nebulosity, it will appear that the nebulosities increase in diameter in a ratio as high at least as the 3⅓ power of the distances from the sun.

“ Dates.	Distance of the comet from the sun.	True diameter of the nebulosity in terrestrial radii.
Oct. 28	1·4617	79·4
Nov. 7	1·3217	64·8
30	·9668	29·8
Dec. 7	·8473	19·9
14	·7285	11·3
24	·5419	3·1 ”

The solar distances are reckoned in terms of the earth's mean distance.

Where comets have tails, this diminution in the dimensions of the head is a matter of obvious necessity. There are, however, more observations wanting before the true law can be determined.

From these principles it does not follow that every body which moves in an eccentric orbit must, necessarily, be accompanied with a tail, or at least with one of a sensible length. Bodies, for example, which do not approach very near the sun; or which, if they do, are large, and capable of combating the force of the solar rays; and those, again, which may be in the opposite extreme, and too weak to make an opposing force sensible, may move round the sun in orbits of great eccentricity, without any of those tail appendages, or at least such as are visible. But in all these cases, could we examine the atmospheres very nicely, it is probable that great changes in their dimensions would become manifest, at very unequal distances from the sun. Experience seems to confirm these conclusions. For it has been observed, that tails are by no means invariable accompaniments to comets; and those which are without them are generally such as do not come very near the sun, or which, like our present visitors of short periods, appear to consist of a mere congregation of vaporous or gaseous matter. On the contrary, they which approach the sun the nearest, and which have the least nuclei, usually put forth tails of the greatest magnitude and splendour. Such was that described by Aristotle, which appeared in an. 4, Olympiad 101; that in 1106; one in 1668; and the celebrated one in 1680; as well as that of our own times, in 1811. All these had very small and obscure heads, but tails exceedingly large and splendid. "It is true," says Newton, "that the comet of 1618, which came out directly from under the sun's beams, with a very large tail, seemed to equal if not exceed stars of the first magnitude; but then abundance of other comets have appeared, yet greater than this, that put forth shorter tails; some of which are said to have appeared as big as Jupiter, others as large as Venus, or even as the moon." In several, indeed, it has been observed, that the larger the comets are, and the farther they keep from the sun in their perihelia, the less their tails are. Exceptions, however, may occur without any violation of theory; a gross or volatile atmosphere will produce a wonderful difference in the phenomena.

Some of the most remarkable phenomena yet observed are those by Sir John Herschel, which we have given in a preceding section. These bodies are so closely watched by astronomers now, and their appearances so minutely recorded, as to leave little doubt that we shall presently understand them much better than at present we do. Meanwhile, we have thought it necessary to make the preceding observations, and regret, for want of facts, that we cannot carry them to the extent we wish.

SUPPLEMENT TO SECT. I, BOOK III.

ON THE MECHANICAL DIFFUSION OF GASES.

Transpiration, or Passage of Gases through Tubes.

MARCH 6, 1847, by the same conveyance by which I received the proofs of the pp. 209 to 256, of this second volume, and the clean first twelve sheets of the same second volume, I received from my friend Charles Hood, Esq., F.R.S, a copy of the fourth part of the 'Philosophical Transactions' for 1846, containing an elaborate paper with new experiments on the diffusion of gases, by Professor Graham. The fourth part had been published only one or two days before.

In this paper Mr. Graham has repeated many of his former experiments under more accurate and favorable circumstances, and has instituted new ones, both on what he calls "*Effusion*" and "*Transpiration*.' As anticipated, pp. 13 and 57 of this volume, he has made nothing out of his supposed new property of "*Transpiration*." After trying the phenomena in a variety of ways and tempting nature to gratify him with the discovery of some new property, he observes, p. 574, that "transpiration has no uniform relation to the density of the gases." In an addition to the Memoir, p. 587 (dated February 1847), while the article was going through the press, he adds, "I have not yet succeeded in reconciling" transpiration "with any physical law." This is as anticipated in p. 13 of this volume. The difference between the flowing of gases through tubes and through mere thickless diaphragms perforated, is the consequence, very probably, of a difference in the figures of the particles of the gases, and has nothing to do with their specific gravity. If there is any property of the gases to which the phenomena of "transpiration" is more allied than another, it is not unlikely to be their disposition to chemical union. Mr. Graham has observed that the material of the tubes has no influence on the rate of "transpiration."

However, though the Professor has failed as I fully expected he would in this inquiry, his experiments are not without interest. He has shown that the comparative retarding effects of tubes and different gases, has, after a certain length of tube no influence, and that the ratio of the times of passing equal volumes of different gases through the same tube is constant.

That the anomalies which produce what Mr. Graham calls "transpiration" are the effect of a friction peculiar to each gas, is rendered probable from the fact that the "effusion" of equal volumes of atmosphere under different tensions into a nearly sustained vacuum, is not quite equal; the times are sensibly longer with less tensions. However, these prolongations of time may possibly be explained by the rarefied air, in what he calls his "aspirator," returning in a greater proportional quantity into the reservoir occupied with a light tensioned air, than with a heavy.

The law that seems to regulate the "transpiration" of equal volumes of air of different tensions, through very small tubes, is, that the times are inversely as the tensions of the air, other things alike. Did the times follow the laws of effusion through apertures in thickless plates, they would be equal. There is, therefore, a retardation through long tubes which produces an effect greater the less the elasticity of the air, and greater the longer the tube; so as to make the quantity of air delivered in a given time as the square of the elasticity of the air divided by the length of the tube nearly. If these facts hold with tubes of considerable bore, as from Mr. Graham's experiments one would infer they do, they tell heavily against the principle of the atmosphere railway, by showing that the friction of the air is not only greater as the length of the tube is greater, but is greater the more the air is rarefied. Engineers who have advocated the atmospheric system have held the reverse opinion with regard to the last important property.

Another curious fact with regard to the "transpiration" or passage of air through tubes is, that the higher the temperature the slower a given volume passes, instead of, as in effusion, faster.

Some of the gases transpire much more slowly than others. Hydrogen is an eminent instance of suffering great retardation and having slow transpiration; but carburetted hydrogen gains upon hydrogen, and suffers less friction. Carbonic acid is still less affected. In each case the dose of carbon lessons the amount of friction, illustrating what has been previously observed, namely, that chemical combination has a good deal to do with the phenomena of transpiration or with the friction in passing through tubes. Thus, pure hydrogen, which combines in a greater variety of doses with other bodies than oxygen does, is more retarded than oxygen. Oxygen again, whose number of combinations with other bodies exceeds that of nitrogen, is more retarded than nitrogen. See 'Phil. Trans.' for 1846, p. 597, compared with p. 586.

Unfortunately it is next to impossible to procure a tube of a great length of such uniformity of bore as to be able to compare the effect of length on the times of effusion. That would enable us at once to settle the important question as to whether friction upon any one gas is not the sole cause of the difference between effusion and transpiration.

The law that appears to result from Mr. Graham's experiments is the following:

$$\tau = \frac{aVT(L+b)}{E} = \frac{aQT(L+b)}{E^2} \quad . \quad . \quad . \quad (1)$$

in which τ is the time of transpiration; V the volume transpired without regard to the elasticity; T the true temperature; E the elasticity of the air in the jar to be transpired; L the length of the tube; Q the actual quantity or weight of air, or volume under a given pressure and temperature transpired; a a constant depending on the diameter of the bore of the tube, the friction peculiar to each air, and other properties; b a constant to be determined.

This formula is of course empirical, suggested by the experiments, which it seems to answer very well.

For example, taking Mr. Graham's experiments (pp. 598, 599) with the capillary tube *B*, he finds when the tube was 31·5 inches long, it transpired one cubic inch of air in 12·6 seconds; when in the same position it was broken off to 14·5 inches, it transpired the same cubic inch in 6·4 seconds; and when reduced to seven inches—I presume in the same position—the time of transpiration was abridged to 3·4 seconds. Now if we put $b = 2·5$ inches, $L + b$ in the three cases will be 34, 17, and 9·5 inches; and

34 : 17 : : 12·6 : 6·4; and 34 : 9·5 : : 12·6 : 3·5;

the one fourth proportional exactly coinciding with the second time observed, and the other fourth proportional 3·5 differing but the tenth of a second from the observed 3·4.

The law, with regard to the elasticity, he has himself settled in experiments from a half atmosphere tension to two atmospheres.

Again as to the temperature. He says, p. 593, that he transpired a given volume of air by the mean of three experiments through a glass capillary tube, 39·4 inches long, in 794 $\frac{2}{3}$ seconds at the temperature of 41° Fahr., giving $T = 1008{\cdot}3$; and with the temperature at 58°, corresponding with $T = 1026{\cdot}7$, an equal volume was transpired through the same tube in 814 seconds. Now

$$1008{\cdot}3 : 1026{\cdot}7 :: 794{\cdot}67 : 809{\cdot}17$$

or 4·8 seconds beneath the observed time. Considering the temperature is given to degrees only, this is a pretty close agreement.

We may, therefore, say that the formula (1) represents facts very closely, and excepting in so far as the peculiar character of each gas and tube requires the modification of a and b, it may be considered a physical law, which Mr. Graham wanted to elicit.

Moreover, since by our laws of airs $E \propto T^2$

$$\tau = \frac{aV}{bT}$$

when $L = 0$. This theorem is precisely the same in form as (2) p. 7, or (4) p. 27; the constants a, b, comprehending the $\sqrt{G}$ and other matters depending on the diameter of the aperture and other things.

There is, therefore, evidently a connexion and a very close one, as I suspected, between the phenomena of transpiration and effusion. In fact they are the same thing, and governed by the same laws, with the exception that effusion, strictly speaking, is free from friction, and transpiration includes it, in quantity peculiar to each gas. This friction, as stated before, I strongly suspect to be allied to the powers of chemical combination of the gases and the figures of their particles; but more experiments are wanting to determine this point.

From (1) we may obtain the actual velocity with which the air issues out of the tube. For since $Q=Ev$, in which v is the velocity of the issuing air,

$$v=\frac{E\tau}{aT(L+b)} \quad . \quad . \quad . \quad (2).$$

That is, the velocity of the air is as its elasticity directly and the product of its true temperature into the length of the tube plus a given quantity according to the bore of the tube, &c. inversely. Or the temperature being the same, the velocity is as the elasticity directly and the length of the tube plus a given quantity inversely.

Hence in the atmospheric plan of railways the difficulty of the air following up the exhausting pump, is increased both by the tenuity of the air in the tube and by the distance the pump is off from the travelling piston. And in the same way, unless there is ample space behind the travelling piston to supply the vacuum it occasions behind it, the after pressure loses in power as the length behind the piston increases. The latter defect may be remedied to a great extent, but the former cannot; it is one of Nature's laws impressed upon matter.

It may be possible to connect the influence of the breadth of the tube with the length, but without special experiments to guide one, the results could not be depended on, and therefore I refrain from the attempt.

Effusion of Air of Different Densities.

Upon the subject of transpiration we are left without any law, properly speaking, by Mr. Graham; but on effusion he has been very successful.

He has clearly proved that equal volumes of air, of different densities but at the same temperature, flow into a vacuum in the same time, allowance being made for the imperfections of the vacuum he was able to keep up. His experiments ranged from air of $\frac{1}{4}$ of an atmosphere tension to 2 atmospheres. This is perfectly conformable with our (2) p. 7; for it being the same air $\sqrt{G}$ is constant as well as the other quantities.

This is one part of the theorem I was anxious to see proved, the other part, namely, that respecting the rates of effusion of different gases, and depending on $\sqrt{G}$, Mr. Graham had long before proved; and fortunately it came into his head last November to try.

The Effect of Temperature on Effusion.

He suffered a quantity of dry air to effuse through a very small, irregular, "hatchet-form" hole, into a two-pint jar of 72·54 cubic inches capacity, and noted that the vacuum barometer sank from $28\frac{1}{2}$ inches to $23\frac{1}{2}$ in 531·66 seconds by the mean of three experiments, the thermometer being 41° Fahr. and height of the barometer 29·616 inches. "The room being heated up to 52°, the time of effusion of an equal volume" by the mean of three experiments was found to be 526 seconds. Now the true temperatures corresponding with 41° and 52° are by our Table, p. 286, Vol. I, &c. 1·0093 and 1·0206, that of 32° being 1. The reciprocals of these numbers are ·9908 and ·9798; and

531·66 : 526 : : ·9908 : ·9803.

These two numbers are very close, being

computed .	·9908 . .	·9798
and observed .	·9908 . .	·9803.

"With the barometer at 30·186 to 30·150 inches, experiments were again made on the effusion of the same volume of dry air at 38°, 48°, and 58°, four hours elapsing between each set of experiments" "to bring up the room and apparatus to a uniform and steady temperature. In three experiments at each temperature" the time of effusion was in seconds.

	Seconds.		Seconds.		Seconds.
At 38° .	526,	at 48°	520,	at 58°	515
„	527	„	521	„	516
„	526	„	520	„	515
means	526·33		520·33		515·33.

The true temperatures are

1·0062 1·0165 1·0267,

and their reciprocals

·9938 ·9838 ·9739.

The mean times of effusion deduced from the numbers of Mr. Graham, as in the preceding case, are

·9938 ·9825 ·9730.

The five observations by Mr. Graham thus confirm the only remaining part of our theorem. It will be observed again, in the second and third experiments, how very near the experimental numbers approach the theoretic. Had the effusions been into a perfect vacuum, there is no doubt that they would have coincided. As it was, the rise of temperature in the "aspirator," observed by Mr. Graham, tended to shorten the times, by increasing the elasticity in the aspirator.

The complete experimental verification of the theorem (2) p. 7, has higher claims to our attention than the simple fact of proving a theoretical deduction.

It will be observed, that the effusion time, that is, the time of a given volume oozing out, is, at the same temperature, constant for all elasticities. So that ten or twenty times the same quantity of gas will run out in the same time, provided it be first contained in the same volume. But the moment the temperature is changed, it not merely alters the rate of effusion but it will introduce a new law, and put the same gas upon the footing of a different gas. Thus, if we suppose gas to be expanded by a diminution of the pressure to twice the volume, and another volume equal to the first be expanded by an increase of temperature to the same double volume, the air of the higher temperature, though it contains precisely the same number of particles, will not flow out in the same but a much quicker time than the other; in the ratio for instance, as Mr. Graham has observed, of the square root of the new density to the square root of the primitive density; or, which is the same, as the square root of the first Fahr. temperature plus 480° to the square root of the second Fahr. temperature plus 480°; that is, as the first true temperature is to the second. This experiment may therefore be looked upon as a demonstration of our law of heat.

It may be observed, that in the experiments cited, the ratios of the Fahr. temperatures are very nearly those of the true; but the manner in which Mr. Graham has evolved the law, that is, by comparing the densities with those of air at 32° Fahr., has rendered the connexion between the densities and effusions quite manifest, and on that depends the law of temperature.

Calculation of the Diameter of the Aperture.

My attention has been drawn to these new experiments, to see if it is possible to test the theorems in Proposition IV, and particularly (4) p. 27. Considerable uncertainty attends this, from the practical difficulty of measuring accurately such small apertures as those of the size employed, that is the 250th or 260th of an inch diameter. Professor Graham has taken great precautions to ensure accuracy in his new experiments. The perforated plate he used was "thin platinum foil $\frac{1}{765}$th of an inch in thickness with a circular aperture $\frac{1}{260}$th of an inch in diameter, as measured by Mr. Powell by means of a micrometer." The quantity of air effused, as far as it appears was 65 cubic inches, which went into an aspirator "maintained vacuous, or as nearly so as possible, by uninterrupted exhaustion." (p. 585.) The measure of gas effused was determined by the capacity of a globular jar, "between two marks, one on each of its tubular axes;" and the jar was itself "supported vertically over the water of a pneumatic trough." Unfortunately the total capacity of this jar is not anywhere, as far as I have discovered, stated, a circumstance of little moment in this experiment, but indispensable in others made with the apparatus.

The specific gravity of the atmosphere being unity, (4) p. 27 becomes $1{\cdot}09\ T\tau = \frac{V}{A}$ in which V, A, and T must be in the same denomination, and as the 1·09 and T are feet, τ being seconds, V and A must be given in feet. From not noticing this an error exists in the calculation given p. 28; the aperture is too large. If we suppose the aperture to be a circle whose diameter is δ, $A = \frac{\pi\delta^2}{4}$ and A given in inches is, when reduced to feet, $\frac{V}{12A} = \frac{V}{3\pi\delta^2}$,

V being still the cubic inches. Therefore

$$\delta = \sqrt{\frac{V}{1{\cdot}09 \times 3\pi T\tau}} \quad . \quad . \quad . \ (3).$$

In the first series of experiments by Mr. Graham (p. 586), $\tau = 865{\cdot}5$, T corresponding with 68° F is 1036·8, and $V = 65$, as far as can be gathered from the statement. Substituting these numbers in (3) we have in inches

$$\delta = \sqrt{\frac{65}{1{\cdot}09 \times 3 \times 3{\cdot}14159 \times 1036{\cdot}8 \times 865{\cdot}5}} =$$

$$\frac{1}{380} = {\cdot}00263$$

instead of $\frac{1}{260} = {\cdot}00385$ inch, as said to have been measured by Mr. Powell. The latter exceeds the former by the ·00122 inch, a very small quantity it is true, but too much one would think to escape micrometrical detection. The error is of a character to show too slow a transit of the air. From the phenomena of friction observed in the passage of gases through tubes it is probable, thin as the metal is, that there is some resistance and some retardation to the flowing of the air. And since the aspirator is not perfectly vacuous a prolongation of the time takes place on this account; for the actual quantity of air effused is not more than equivalent to the quantity that would be effused at a tension equal to the difference of the tensions of the air in the jar and in the aspirator. It is probably less than this, inasmuch as it is not likely that the particles of air coming in and going out wholly pass each other without interruption. Hence on this account the time will be prolonged.

If we make a second computation from "section 5" of Mr. Graham's experiments we have $\tau = 869{\cdot}5$, and $T = 1031{\cdot}8$ corresponding to 63°F.; all other quantities being as before. Hence

$\delta = {\cdot}0026557 = \frac{1}{376{\cdot}5}$ nearly the same as before.

The reciprocals 263 and 265 approach so near the measured 260 as to suggest the idea whether there has been any mistake in taking the reciprocal for the number.

We think a sufficient concord between the experiments and theory has been established to show *that all gases flow into a vacuum with the same velocity as they conduct sound whatever be their elasticity; that, other things alike, the same number of particles of every gas will pass through the same aperture in the same time;* and that *the volume occupied will be as the square of that number.*

The supposition, therefore, of a gas rushing into a vacuum faster, because its elasticity is greater, is erroneous. More particles enter the vacuum in proportion to the elasticity of the medium, but the absolute velocity of the particles is the same if the temperature is the same. On the temperature only does the velocity of the particles depend.

Effusion of Mixed Gases.

Four series of experiments of the effusion of mixtures of hydrogen with air, oxygen, carbonic acid, and nitrogen are given, made with the same apparatus; but as neither the capacity of the jar nor the time the effusion had been going on before the observation commenced is given, the results cannot be subjected to theoretical comparison. It would appear, however, by Mr. Graham's computations, that the times "calculated from * * * the square roots of the densities of the respective mixtures" almost perfectly coincide with his observations. This is at variance with his observations in the first part of his 'Elements of Chemistry,' published last November, which are discussed, and doubts of their truth expressed, in pages 18 and 19 of this volume.

The following are his new observations, which it appears were made three months since his 'Elements' were published. They are a valuable set of experiments, and appear to have been carefully and skilfully conducted. It is to be regretted that he has not given us the data alluded to respecting the size of the jar, the quantity of gas in the jar at the commencement of the experiments, and the length of time the effusion had gone on before the observations were commenced.

TABLE of the Times of Effusion of various Gases and Gaseous Mixtures into a sustained vacuum, through a circular aperture $\frac{1}{260}$ inch diameter, in a plate $\frac{1}{765}$ inch thick.

	I. Seconds	II. Seconds	Mean Seconds	Ox.=1. Seconds	Cal.time Ox.=1.
SECTION 1.—Barometer 30·396; Thermometer 68°; *T* = 1036·8.					
Oxygen . . .	909	909	909	1·0000	
Air	866	865	865·5	0·9521	
Hydrogen . . .	242	242	242	0·2662	
Carburetted Hydrogen	624	622	623	0·6853	
Carbonic Oxide . .	849	850	849·5	0·9345	
Nitrogen . . .	850	851	850·5	0·9356	
Air	864				
Carbonic acid . .	1053	1051	1052	1·1573	
SECTION 2.—Barometer 30·288; Thermometer 66°; *T*=1034·8.					
Oxygen . . .	912	912	912	1·0000	
Air	868	867	867·5	0·9512	
Hydrogen . . .	240	240	240	0·2631	
25H+75 Air . .	756	756	756	0·8289	0·8328
50H+50 Air . .	633	633	633	0·6940	0·6951
75H+25 Air . .	483	483	483	0·5296	0·5224
80H+20 Air . .	444	444	444	0·4868	0·4805
90H+10 Air . .	358	358	358	0·3925	0·3830
95H+5 Air . .	309	309	309	0·3388	0·3296
Air	864				
SECTION 3.—Barometer 30·219; Thermometer 66°; *T*=1034·8					
Oxygen . . .	912	910	911	1·0000	
Air	867	865	866	0·9506	
Hydrogen . . .	239	241	240	0·2634	
12·5H+87·5O . .	853	855	854	0·9374	0·9396
25H+75O . .	796	796	796	0·8737	0·8750
37·5H+62·5O . .	733	733	733	0·8046	0·7990
50H+50O . .	661	661	661	0·7255	0·7289
62·5H+37·5O . .	586	586	585·5	0·6427	0·6435
75H+25O . .	501	501	501	0·5499	0·5449
80H+20O . .	460	461	460·5	0·5055	0·5000
90H+10O . .	368	368	368	0·4039	0·3954
95H+5O . . .	312	312	312	0·3424	0·3309
Air	860				

SECTION 4.—Barometer 29·673; Thermometer 64°; $T=1032·8$.

	I. Seconds	II. Seconds	Mean Seconds	Ox.=1. Seconds	Cal.time Ox.=1.
Oxygen . . .	914	913	913·5	1·0000	
Air	870	869	869·5	0·9518	
Carbonic oxide . .	854	853	853·5	0·9343	
Hydrogen . . .	238	239	238·5	0·2610	
25H+75CO . .	749	748	748·5	0·3193	0·8199
50H+50CO . .	626	626	626	0·6852	0·6848
75H+25CO . .	478	478	478	0·5231	0·5155
80H+20CO . . .	445	445	445	0·4871	0·4745
90H+10CO . .	360	360	360	0·3940	0·3793
95H+5CO . .	314	314	314	0·3437	0·3213
Air	870				

SECTION 5.—Barometer 29·500; Thermometer 63°; $T=1031·8$.

Oxygen . . .	915	914	914·5	1·0000	
Air	870	869	869·5	0·9508	
Nitrogen . . .	855	854	854·5	0·9344	
Hydrogen . . .	241	241	241	0·2635	
25H+75N . .	753	753	753	0·8234	0·8213
50H+50N . .	631	631	631	0·6899	0·6859
75H+25N . .	480	478	479	0·5238	0·5163
80H+20N . .	442	442	442	0·4833	0·4752
90H+10N . .	359	359	359	0·3925	0·3797
95H+5N . . .	309	310	309·5	0·3379	0·3215
Hydrogen . . .	241				
Air	869				

The numbers in the 5th and 6th columns are the Professor's numbers. How they were computed does not clearly appear. I am inclined to think that they are the results of computations, on the principle that the two mixed gases effused proportionably to the volumes of their mixtures; that is so as always to flow out in the same relative volumes as they are mixed, and of course to retain the same constituent proportion of volumes in the jar. If such be the case it is incorrect. For the lighter gas will escape much faster than the heavier. But the quantity let out may be such, in proportion to the contents of the jar, as to give a time nearly corresponding to the times due to each gas. That I expect is the case in these experiments; for the observed and computed times in the 5th and 6th columns agree very closely. The near agreement may arise either from nearly the whole being effused, or a very small quantity in proportion to the whole quantity by (*d*), *Cor.* 1, Proposition B, p. 16.

Should the learned Professor resume this subject, it is to be hoped that he will carefully note the primitive volume of the mixture, with the proportional volumes of the two gases, the time the effusion goes on before the experiment commences, and the total quantity actually effused in addition to the quantity between certain marks.

It would be advisable, if possible, for the experiment to begin the moment the effusion begins, and to have several marks at equal divisions on the magazine, so as to be able to have the corresponding times at which equal volumes escape, as the experiment goes on.

After the experiment is ended the quantities of each gas remaining in the magazine should also be ascertained.

It would be very desirable, if it was possible, to obtain tubes of considerable lengths with uniform bores, and to cut off from them extremely thin rings so as to compare the effect of length on the times of effusion of the different gases. Mr. Graham, p. 617, seems to be on the right scent: for he supposes the phenomena of transpiration to be the effect of "friction;" and with respect to the various gases he conceives the difference "to depend on a constitutional difference in the gases themselves." In that he is right, but the "constitutional difference" is merely a difference in the figures of the particles, and has nothing to do with the "heat" of the gas "latent" or "sensible."

In future experiments it would be advisable, if possible, to try the effect of the size of bore of the tube, in order that we may connect that with the length of the tube in the formula (1), p. 352. That is wanting to complete the theory of transpiration. What law experiment may give I know not, but I should not be surprised to find the effect of the bore inversely as its diameter on the same air, all other things alike. This is, however, mere conjecture and not deduced from any physical or analytical investigation.

APPENDIX

TO PROPOSITION III, SECTION 1, BOOK IV, p. 240.

A Lemma is given, p. 239, which was intended to be applied in the solution of the Problem in Proposition III, but owing to some oversight and change in the MS. it was omitted. We shall, therefore, supply that omission in the shape of another solution.

If we consider the central agitation to be propagated through the medium, and that v is the velocity of a particle of the medium at a distance x from the centre, v may be taken as the constrained effect of the force at the said distance x, after being diluted in some way by the peculiar actions of the particles on each other through which it has passed. Therefore if f be the force on the particle after it is so diluted, we shall have by the Lemma

$$-dv = \frac{vfdt}{\mathrm{v}}$$

v being the normal effect and a constant. But f is equal to the force A of central agitation reduced to the individual particle; that is, as the intensity is not, from the nature of the collison of hard bodies, impaired by the distance, except as by being spread over a greater space and among more particles. It is as $\frac{A}{nx^2}$.

Again, as there is no force soliciting the particles while in motion, the force on them can only be at the collisions occurring at the extremity of their excursions; and the time dt, therefore, becomes measured by the number of these collisions. That is, dt is expounded by $n^{\frac{1}{3}}dx$. So that the equation becomes

$$-dv = \frac{vAdx}{\mathrm{v}n^{\frac{2}{3}}x^2};$$

v indeed merges into A.

When this celebrated problem was many years ago under investigation, there were some five or six different solutions I succeeded in giving it; but no notes having been taken of more than those in the Proposition, they have slipped my memory.

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